4

Research Report CCS 590

MORE ON BREAKING UP BELL

by

A. Charnes
W.W. Cooper
T. Sueyoshi*

CENTER FOR CYBERNETIC STUDIES
The University of Texas
Austin, Texas 78712

4

Research Report CCS 590

MORE ON BREAKING UP BELL

by

A. Charnes
W.W. Cooper
T. Sueyoshi*

March 1988

*Ohio State University

The research for the papers in this collection by A. Charnes, W.W. Cooper and T. Sueyoshi was partly supported by National Science Foundation Grants SES 8520806 and SES 8408134, Office of Naval Research Contract N00014-86-C-0398, and U.S. Army Contract DAKF-15-87-C-0110, with the Center for Cybernetic Studies at The University of Texas at Austin, Austin, Texas 78712-1177. It was also partly supported by the IC2 Institute at The University of Texas at Austin and by Ohio State University. Reproduction in whole or in part is permitted for any purposes of the U.S. Government.

CENTER FOR CYBERNETIC STUDIES

A. Charnes, Director

College of Business Administration, 5.202
The University of Texas at Austin
Austin, Texas 78712-1177
(512) 471-1821

TABLE OF CONTENTS

This paper consists of the following five with 1, 3, 4, 5 by Charnes, Cooper and Sueyoshi and 2 by Evans and Heckman.

1) "Goal Programming/Constrained Regression Review of the Bell System Breakup," Management Science 34, No. 1, Jan. 1988, by A. Charnes, W.W. Cooper and T. Sueyoshi.

2. "Rejoinder. Natural Monopoly and Bell System: Response to Charnes. Cooper and Sueyoshi," Management Science 34, No. 1, Jan. 1988 by David S. Evans and James Heckman.

3. "A Preface and Post Script to Evans and Heckman Studies for the Bell System Breakup," CCS Report 588, The University of Texas at Austin, Center for Cybernetic Studies, Jan. 1988, by A. Charnes, W.W. Cooper and T. Sueyoshi.

4. "Exhibit I: A Guided Tour of A Family of Studies on the AT&T Breakup," CCS Report 580, The University of Texas at Austin, Center for Cybernetic Studies, May 1987, by A. Charnes, W.W. Cooper and T. Sueyoshi.

5. "Exhibit II: On Errors in the Evans and Heckman Bell System Breakup Studies," CCS Report 550, The University of Texas at Austin, Center for Cybernetic Studies, October 1986, by A. Charnes, W.W. Cooper and T. Sueyoshi.

MANAGEMENT SCIENCE
Vol. 34, No. 1, January 1988
Printed in U.S.A.

A GOAL PROGRAMMING/CONSTRAINED REGRESSION REVIEW OF THE BELL SYSTEM BREAKUP*

A. CHARNES, W. W. COOPER AND T. SUEYOSHI
Center for Cybernetic Studies, University of Texas, Austin, Texas 78712-1177
College and Graduate School of Business, University of Texas, Austin, Texas 78712-1175
School of Public Administration, The Ohio State University, Columbus, Ohio 43210-1399

The recently implemented court decision to break up Bell (= American Telephone & Telegraph Co.) to accord with U.S. anti-trust laws represents a highly significant policy decision which is proving to be influential in other countries as well as the U.S. The telecommunication industry is of such size and importance that even relatively small economies that might be lost with Bell's breakup as a "natural monopoly" could involve substantial welfare losses to consumers and producers. Studies commissioned by the U.S. Justice Department that approached this topic by econometric methods reported that the evidence failed to support the contention that Bell was a natural monopoly. Here a goal programming/constrained regression, as developed in the Management Science literature, uses the same functional form and the same data but nevertheless reverses the main findings of the econometric studies in every one of the 20 years covered. This kind of difference in results obtained by two different methods of analysis points up a need for drawing on persons from different disciplines *who are capable of checking each other's methodologies* when important policy decisions may be influenced by results that depend on the methodologies that these disciplines customarily use. Advantages of doing this are further illustrated by data deficiencies that escape detection by the econometric methods employed in the Justice Department-commissioned studies.
(NATURAL MONOPOLY; PRODUCTION FUNCTION; EFFICIENCY; TRANSLOG FLEXIBLE FUNCTIONS; ECONOMETRIC MODELS; GOAL PROGRAMMING/CONSTRAINED REGRESSIONS)

1. Introduction

This paper constitutes a study in the use of alternate methodologies and the different results that may emerge from their application to exactly the same body of empirical data. The center of attention will be on an empirical analysis of the natural monopoly issue as reported in *Breaking Up Bell*. Edited by Evans (1983), this book is a compilation of economic analyses and reports prepared for the U.S. Justice Department to guide or support the Government's case. To quote from its accompanying advertisement:

> *Breaking Up Bell* is a compilation of nine essays written by top-notch economists of the 'Chicago School' who were consultants to the Justice Department during U.S. versus AT&T [and the book thus] offers authoritative economic analysis of one of the most celebrated antitrust cases in history. . . ."

Chapter 10 by Evans and Heckman in Evans (1983) is especially important not only because it addresses the central economic issue of whether Bell constituted a "natural monopoly" with associated cost savings and benefits that might be lost with a breakup but also because it is almost the only one of the reports in *Breaking Up Bell* where a systematic and detailed empirical inquiry into these issues is undertaken.

To trace the implications of evidence that was employed we use the same functional form and the same concepts as Evans and Heckman. This does not mean that we agree

* Accepted by Arie Y. Lewin; received May 1985. This paper has been with the authors 3½ months for 3 revisions.

0025-1909/88/3401/0001$01.25

with these concepts and choices of functional forms for such policy analysis, or that we would ourselves follow this route in other respects. Our purpose is rather to highlight possible sources of misunderstanding that may occur when only one kind of methodological and conceptual approach is employed in deriving estimates and making inferences from empirical data.

Almost all of the analytically oriented studies (pro and con) in the Bell case were undertaken by economists and econometricians using techniques such as statistical regressions and index number constructions that constitute the methodologies commonly employed for empirical analysis by economists.[1] To highlight what is involved in such choices of methodologies we shall use a different approach based on the methods of goal programming/constrained regression, as developed in operations research and management science (Charnes, Cooper and Ferguson 1955 and Charnes, Cooper and Sueyoshi 1986). Using the same data and employing the same functional forms as Evans and Heckman we will then arrive at diametrically opposite conclusions by simply choosing this different methodology.

The possible biases involved in methods of analysis and estimation are closely allied to choices of the disciplines to be used. These methodology choices and their consequences are difficult to detect and discover by outsiders (and even by persons within a particular discipline) unless cross-checked by recourse to methodologies associated with other disciplines. This is the main message we seek to convey: at least when large issues of policy are to be guided by the resulting inferences, it will generally be prudent not to rely on only one discipline (e.g., economics-econometrics) but also to have recourse to other disciplines which employ different methodologies.

In this case we use goal programming as a method of statistical estimation. That is, we use goal programming for purposes like those described in Charnes, Cooper and Ferguson (1955) which introduced the concepts of goal programming as a method for effecting statistical estimates and which has now been extended and elaborated in a variety of ways. This OR/MS methodology is used here to check results from the SUR (Seemingly Unrelated Regressions) methodology of econometrics as used by Evans and Heckman, a standard econometric estimation method introduced by A. Zellner (1968) and subsequently extended and developed in a variety of ways in the econometrics literature. See Phillips (1985).

The principle of "methodology cross-checking" which we are suggesting in this paper is not anti-econometric methodology. The proposed cross-checking should be applied symmetrically. It is equally prudent to use econometric (and other) methodologies to cross check results that might be secured from OR/MS approaches as well as *vice versa*. To avoid the possibility of simply arriving at differences in a willy nilly fashion by recourse to a wide variety of disciplines, however, we add the proviso that the analysts utilized for such studies should be capable of understanding each other's methodologies and be required to use this kind of understanding in formulating the way the issues should be addressed. Cross checks can then be obtained in a meaningful and coherent manner and will help to guard against one-sided inferences from undue reliance on only one of the methodologies of a particular discipline while, at the same time, avoiding confusion such as might result from simply proliferating disciplines without providing a common basis of understanding of reasons for the differences that may emerge. Other benefits that can emerge from such an approach, as will be seen, extend to cross-checking and validation of data as well as re-evaluating the way the concepts and methodologies drawn from different disciplines are actually utilized.

[1] For an exception see J. R. Meyer *et al.* (1982) although, as noted in a letter from Professor Meyer, this study was really conducted by an interdisciplinary team. See also the U.S. General Accounting Office report (1981).

2. Multi-Product Cost Model and Data Details

When technical efficiency (i.e., zero waste) can be assumed, the duality theory of Shephard (1970) as employed in economics, makes it possible to proceed from production functions to cost functions, and *vice versa*.[2] The choice of which to employ is only a matter of convenience since this theory ensures consistency in the results.

Evans and Heckman (1983) elected to use a cost rather than a production function in the following "translog" form:

$$\begin{aligned}\ln C = \alpha_0 + \sum_i \alpha_i \ln p_i + \sum_k \beta_k \ln q_k + \mu \ln T + \frac{1}{2}\sum_i \sum_j \gamma_{ij} \ln p_i \ln p_j \\ + \frac{1}{2}\sum_k \sum_r \delta_{kr} \ln q_k \ln q_r + \sum_i \sum_k \rho_{ik} \ln p_i \ln q_k + \sum_i \lambda_i \ln p_i \ln T \\ + \sum_k \theta_k \ln q_k \ln T + \tau[\ln T]^2 \quad (1)\end{aligned}$$

where

$\ln C$ represents the natural logarithm of cost and

p_i = price of the ith input, i = 1, 2, 3 for capital, labor, and materials, respectively.

q_k = quantity of the kth output, k = 1, 2, for local and long distance service, respectively.

T = an index of technological change.

We will also use p and q to represent vectors with p_i and q_k as components and when subscripted by t, the vectors p_t, q_t and scalars C_t, T_t will date the observations from which the coefficients in (1) are to be estimated.

This "translog function" is a so-called "flexible functional form". As noted in Christensen, Jorgensen and Lau (1973), it was introduced to relax restrictions associated with other choices such as, for instance, the widely used Cobb-Douglas[3] forms. The idea is to regard (1) as an approximation to a wide class of functions with special cases such as the Cobb-Douglas form being precipitated out when the data (and the statistical methodologies used) cause this to happen.[4] The large number of parameters available also makes it possible to test hypotheses in a unified manner that is beyond the reach of other, more specialized (hence less flexible), functional forms.

The above translog representation was selected by Evans and Heckman after a study of other functional forms. These other choices are not discussed here. Instead we refer readers to Evans and Heckman (1983) where, as noted on p. 259 ff., the formulation (1) was finally settled upon along with the choice of variables and the index number constructions used. The data we employ are also drawn from Evans and Heckman (1983) and reproduced as Table A.1 in the Appendix to this article.

All data were checked by using the same efficiency assumptions as Evans and Heckman—see Appendix—and these results were also checked by reference to the reports by Christensen (1981) and Christensen, Christensen and Schoech (1981), the sources credited by Evans and Heckman for much of their data. This checking was deemed neces-

[2] Cf. McFadden (1978). See, however, Charnes, Cooper and Schinnar (1982) for strictures on this theory interpreted as transform rather than duality relations in which, as in Tauberian analysis in mathematics, a transform and its inverse transform will not necessarily give consistently correct results over the ranges to be considered.

[3] As shown in Charnes, Cooper and Schinnar (1976), however, the Cobb-Douglas form covers a much wider class of functions than might be supposed. For instance, a natural extension of the Cobb-Douglas function can be used to represent any homogeneous function or, for that matter, any differentiable function. See also the possible uses of piecewise Cobb-Douglas forms in Banker, Charnes, Cooper and Maindiratta (1986) and Charnes, Cooper, Seiford and Stutz (1983).

[4] Subsequent research has shown that these translog representation suffer from difficulties that limit them for many applications. See Caves, Christensen and Swanson (1981), Guilkey and Lovell (1980) and Charnes, Cooper and Schinnar (1982).

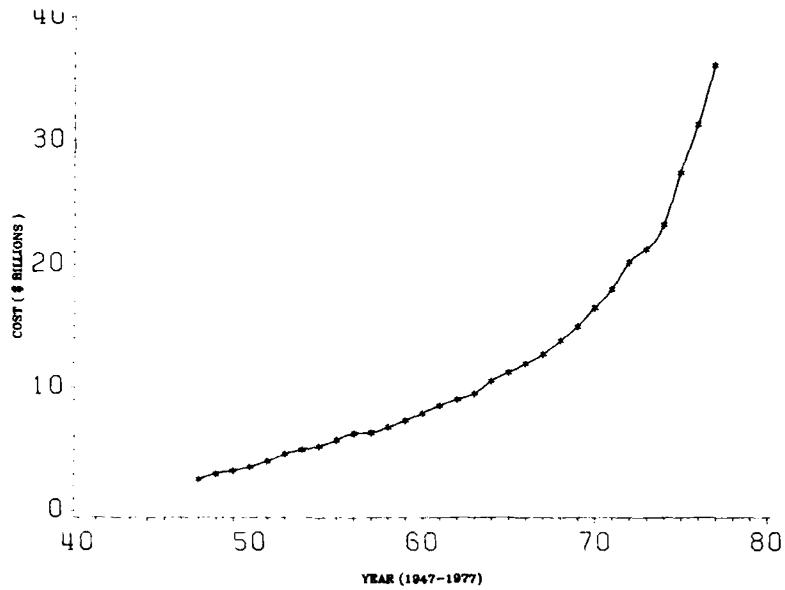

FIGURE 1. Time Trend of Cost.

sary because errors were uncovered in the course of our research which made it apparent that further examinations of the underlying data were needed. The corrections resulting from these data reviews are shown as parenthesized amounts in the body of Table A.1 and still further corrections are provided in Table A.3. Here we use the uncorrected data of Table A.1, i.e., the data as reported by Evans and Heckman, since

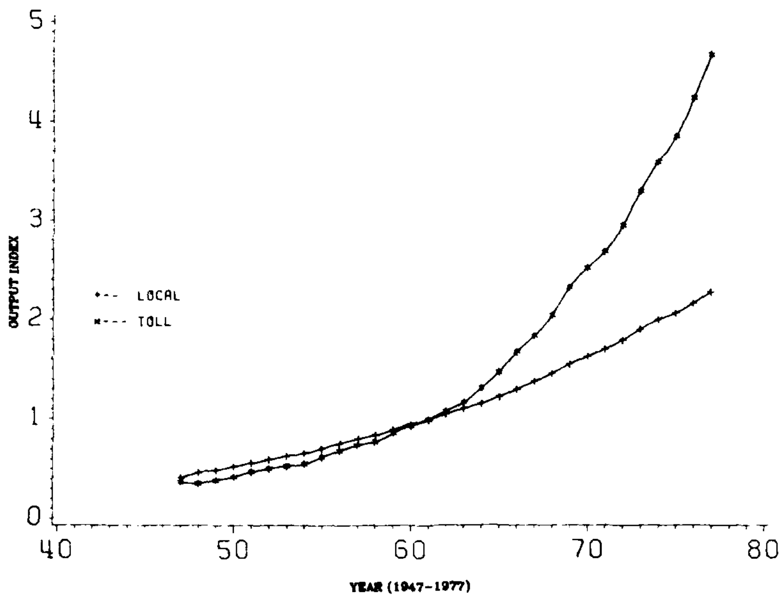

FIGURE 2. Time Trend of Output.

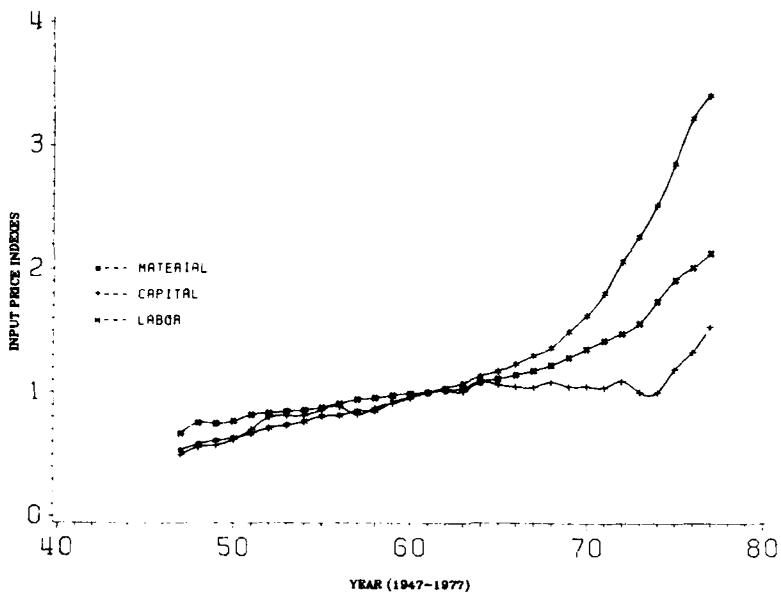

FIGURE 3. Time Trend of Input Prices.

our objective is to show the different results that can emerge with the same functional form, (1), applied to the same data with a different estimating methodology.

These topics (and others) are treated in the sections that follow. We conclude this section by graphically portraying the data for the years 1947–1977 in Figures 1, 2, 3, and 4. These portrayals exhibit unusually smooth behavior, perhaps reflecting some of

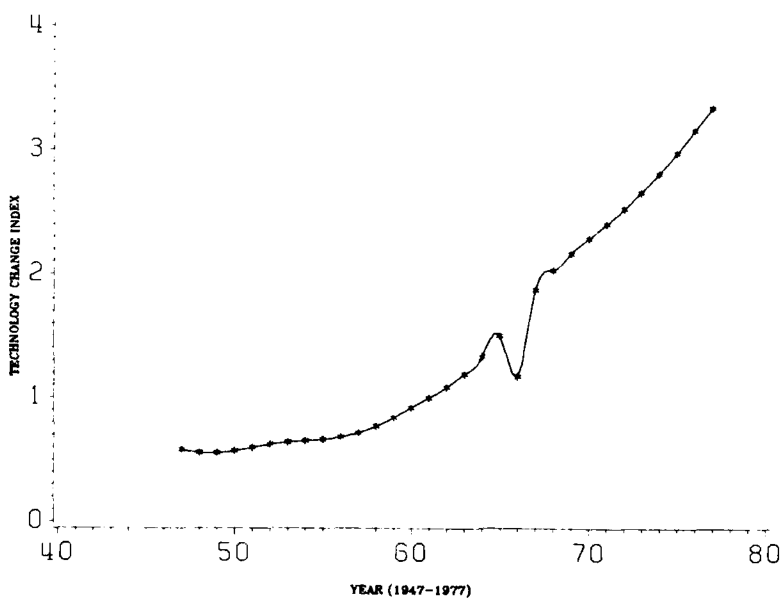

FIGURE 4. Time Trend of Technology.

the index number computations and adjustments that were used. Because these index numbers constituted "data" for these analyses, we also continue to use them and take advantage of the very smooth trends that are apparent in these figures and move rather freely between time and the indicated inputs, outputs, prices, and costs.

Although such graphs were not used by Evans and Heckman or others involved in these studies,[5] they were apparently aware of the way this behavior allowed them to move rather freely up and back over time. They were also probably aware of the striking changes in price behavior that occurred for the inputs during this period. They do not discuss this in explicit detail, however, and they also do not discuss aberrations in the behavior of the technological change index which is apparent over the years 1965–1967.[6]

3. Econometric Model Development

For (1) to be a satisfactory cost function in the sense of economic theory, the parameter values must satisfy a variety of conditions such as "linear homogeneity" and "symmetry" for input prices—as well certain "cost share" conditions which we develop in more detail below. On the basis of their statistical tests, however, Evans and Heckman (1983, p. 263) reach the following conclusion: "We resoundingly reject the homogeneity and symmetry restrictions implied by producer [economic] theory [on the basis of the evidence]." Then they go on to assert: "Like other researchers [in empirical uses of economic theory] we [nevertheless] restrict our cost function estimates to satisfy homogeneity and symmetry." See also Evans and Heckman (1984, p. 620).

Reliance on economic theory in this manner has various advantages in that its implications may provide possibilities for increasing the degrees of freedom needed to make statistically meaningful estimates from empirical data.[7] As Evans and Heckman (1983, p. 140) note, "implications of producer economics provides a great deal of information [sic] which [can be added to the statistical analysis] to increase the degrees of freedom." Thus, assuming that the rejected conditions of homogeneity and symmetry are satisfied makes it possible to reduce the number of parameters to be estimated from 32 to 21 in (1) as follows:

To start, the 32 parameters formally exhibited in (1) for $i = 1, 2, 3$ and $k = 1, 2$ are reduced to 31 by assuming that $\delta_{12} = \delta_{21}$. The following tally then leads to still further simplifications for purposes of econometric estimation as follows:

A. Linear Homogeneity in input prices requires $\sum \alpha_i = 1$, $\sum \gamma_{ij} = 0$, $\sum \rho_{ik} = 0$ and $\sum \lambda_i = 0$. The assumption that these conditions are satisfied makes possible the following reductions:

$\sum_i \alpha_i = 1$ remove 1 parameter,
$\sum_j \gamma_{ij} = 0$ remove 3 parameters,
$\sum_i \rho_{ik} = 0$ remove 2 parameters,
$\sum_i \lambda_i = 0$ remove 1 parameter.

B. Symmetry requires $\gamma_{ij} = \gamma_{ji}$ when $i \neq j$ and satisfaction of these conditions results in a reduction of 3 more parameters.

These reductions result in producing 10 degrees of freedom with 21 parameters to estimate from the 31 observations in Table A.1.

Assuming that the observations are normally distributed (in the statistical sense),

[5] An exception is the Testimony of Christensen (1981) which appears in the "Findings," as part of some 30,000 pages of recorded testimony available in the Telecommunications Library at George Washington University in Washington, D.C.

[6] This technological change index was obtained from Vinod (1976).

[7] See M. W. Reder (1982), however, for a detailed and insightful discussion of some of the dangers that can attend preoccupations with using and preserving the body of economic theory.

Evans and Heckman are then able to effect a variety of statistical tests. In addition to the tests on the economics-econometrics issues referred to in the preceding paragraphs, this testing extended to the central economic issues involved in "Breaking Up Bell"—*viz.*, does the evidence show AT&T to be a "natural monopoly" exhibiting "economies of scale" and/or "economies of scope" that might be lost if the system were broken up?

4. A Goal Programming/Constrained Regression Alternative

Having sketched the econometric modeling strategies employed by Evans and Heckman, we turn to the route we shall follow. Instead of *assuming* that the economic theory of production is applicable to these data we will explicitly introduce constraints to ensure that conditions A and B of the immediately preceding section are satisfied. We utilize the same translog cost function as Evans and Heckman[8] but orient it toward frontier rather than interior point estimates. This enables us to obtain insights into whether the behavior of AT&T costs is consistent with the economically efficient performance assumed by Evans and Heckman.

There are a variety of other conditions implied by the economic theory of production which could not be satisfied because of the data difficulties that are addressed in the Appendix to the present paper. The "cost/share conditions" are a case in point. These shares must satisfy a "summing up" condition in which the total of the shares assigned to each input must sum to 100% of the total costs in each year.

In principle the approach we are using would require introducing constraints to insure satisfaction of this summing up condition in every year.[9] However, errors introduced into the data (see Appendix) made it impossible to satisfy these "summing up conditions" precisely in the present case. We, therefore, employed approximations with accompanying constraints which, as it turned out, satisfied the summing up requirement closely in most cases.

"Goal programming/constrained regression" is the name we accord the model we use to obtain our estimates. See Charnes, Cooper and Sueyoshi (1986). Although statistical in the sense of using mathematical-analytical techniques[10] to obtain estimates from empirical observations, we do not employ statistical tests of significance like those used by Evans and Heckman. We introduce instead a type of sensitivity analysis which (as will be seen) is readily adapted for use in (a) examining the extent to which individual observations may be varied without affecting the coefficient values obtained from these goal programming regressions as well as for use in (b) evaluating the increased or lessened values of the total deviations between regression values and observations when particular constraints are tightened or relaxed.

This path of development is dictated by reasons like the following: Methods for effecting statistical significance tests for such goal programming/constrained regressions are not presently known and their development would not only be difficult but would involve extensive treatments that would likely extend to several papers as research was developed over a long period of time.[11] Moreover, systematic errors in the data, as discussed in the Appendix, would first need to be eliminated in order for such tests to be statistically meaningful. Effecting these adjustments, however, would frus-

[8] More precisely we select one of the pair of translog forms which seem to be preferred by Evans and Heckman on the basis of tests they conducted over a variety of possibilities.

[9] In Charnes, Cooper, Sueyoshi (forthcoming) the data were corrected so that these cost-share constraints could satisfy the summing up condition in every year.

[10] The statistical character of these goal programming estimates has been recognized from the outset, as in the proof of statistical consistency of goal programming estimates given in the Appendix to Charnes, Cooper and Ferguson (1955).

[11] The results reported in Bassett and Koenker (1978) and Koenker-Bassett (1978) are not applicable for the kinds of constrained regressions we are using here.

trate the main purpose of the present paper which is to compare results obtained from our goal programming/constrained regressions with those obtained by Evans and Heckman from the data they used (as reported in Evans and Heckman 1983 and reproduced in Table A.1 of the Appendix to the present paper).

Statistical tests of an elementary variety such as examining the behavior of residuals are used in the present paper. These statistical examinations are extended to prediction tests which are conveniently available for use in the present study because Evans and Heckman did not use data for 1977 and 1978 as provided in Christensen, Cummings and Schoech (1981)—the basic source of the data used by Evans and Heckman and by us in these studies.

5. Model Development

We use the following goal programming/constrained regression which admits only one-sided deviations:

$$\text{minimize} \quad \sum_{t=1}^{n} \delta_t$$

$$\text{subject to} \quad f(p_t, q_t, T_t) + \delta_t = \ln C_t,\ \delta_t \geq 0, \qquad t = 1, 2, \ldots, n. \tag{2}$$

Here $f(\cdot \cdot \cdot)$ is the translog function to be estimated, in the same form as (1), and $\ln C_t$ is the natural logarithm of C_t, the observed total cost in year t. Because the δ_t are all constrained to be nonnegative, the estimated coefficient values must satisfy $f(p_t, q_t, T_t) \leq \ln (C_t)$ for every t. Solving for the minimizing objective in (2) provides coefficient values for our translog function that will yield estimates of total cost that are as close to the observed total costs as these constraints will allow. The thus estimated cost function possesses a frontier (= envelope) property relative to the observed costs. Following Aigner and Chu (1968) we interpret this as an "efficient frontier" with $f(p_t, q_t, T_t) < \ln C_t$ representing some amount of inefficiency, whenever it occurs for any t.

The "producer economics" which Evans and Heckman use defines the production and cost functions so that they will coincide with efficiency frontiers. They use "central tendency" rather than "frontier estimates" in their regression fitting methods, however, so that they must be assuming that their observations scatter about their fitted function in a random fashion—i.e., without contamination by managerial errors or other sources of inefficiency—which allows them to apply these "frontier oriented" concepts to the resulting regression relations. Without such suppositions none of the results of the micro-economic theory of production can be used in the manner employed by Evans and Heckman or by others who have employed similar approaches.

The formulation in (2) provides a possible way of looking at the evidence[12] to see whether such "efficiency frontier" assumptions can be satisfied to a reasonable degree. Other conditions like those listed under A and B in §3, above, must also be satisfied if the thus estimated function is to satisfy the conditions of economic theory. We therefore adjoin constraints to (2) that ensure satisfaction of these conditions.

As already noted, the "cost share conditions" give rise to special problems. We therefore discuss them in more detail as follows.

In customary notation, these conditions are represented as

$$0 \leq S_i = \frac{p_i x_i}{C} \leq 1, \tag{i}$$

[12] The formulation in (2) is not the only way of achieving efficiency (frontier) properties. See Hanoch and Rothschild (1972) for an example of an alternative route that was available at the time the studies we are examining were undertaken. See Charnes, Cooper, Golany, Seiford and Stutz (1985) for still another approach.

where x_i and p_i are the amount and the price of the ith input for each $i = 1, 2, 3$—*viz*, capital, labor, and material, respectively—and

$$\sum_{i=1}^{3} S_i = 1 \tag{ii}$$

since $\sum_i p_i x_i = C$. As indicated in the Appendix the data do not satisfy these "summing up" conditions of (i) and (ii) in any of the years covered.[13]

Evans and Heckman follow a route that is customary in econometric investigations and omit one of the factor shares. This replaces (ii) with

$$\sum_{i=1}^{2} S_i \leq C. \tag{iii}$$

Here, following Evans and Heckman, the factor share for material is to be obtained as the difference between the two sides of (iii) in each year.[14] The condition (iii) is violated in some years, however, and so with this route blocked by data inconsistencies we turn to surrogate measures which also provide insight (and some control) on other economics aspects of the resulting estimates.

To explain what was done we turn first to the condition $\partial^2 C/\partial p_i^2 \leq 0$ which is necessary for concavity in each of the three input prices. See Varian (1984, p. 45). We weakened this condition, however, to

$$\frac{\partial \ln C}{\partial \ln p_i} = \alpha_i + \sum_j \gamma_{ij} \ln p_j + \sum_k \rho_{ik} \ln q_k + \lambda_i \ln T \geq \gamma_{ii} \tag{iv}$$

so that we could also relate this condition to conditions on the "own price" elasticities of the inputs. See Appendix.

The expression on the left in (iv) is obtainable by differentiating (1) with respect to $\ln p_i$. We also have

$$\partial C/\partial p_i = x_i \tag{v}$$

by virtue of "Shephard's lemma."[15] Hence,

$$\frac{\partial \ln C}{\partial \ln p_i} = \frac{p_i}{C}\frac{\partial C}{\partial p_i} = \frac{p_i x_i}{C} = S_i \tag{vi}$$

so that via (iv) we can translate these cost share conditions into a constraint that is readily usable with our translog cost function.

We next imposed the following constraints on the γ_{ii} values on the right-hand side of (iv):

$$\gamma_{ii} \geq -S_i^{*2} \quad \text{where}$$
$$S_i^* = \min_t S_{it} \quad \text{for} \quad t = 1, 2, \ldots, n. \tag{vii}$$

This was done partly to control the own price elasticities which are related to these γ_{ii} values,[16] and also to make it possible to examine the behavior of the S_i estimates since, as already indicated, potentially serious questions are present as to the applicability of Shephard's lemma in the present study.

[13] As discussed by Sueyoshi (1986), repeated returns of "no solution" from computer runs from a large variety of different modeling efforts led, finally, to the discovery of these data inconsistencies.

[14] See Bewley (1986) for a general discussion of this problem and its various treatments.

[15] As obtained from Shephard's (1970) duality theory which plays a prominent role in modern microeconomic theory. For cautions on the applicability of this theory under conditions where, *inter alia*, capacity limitations are present, as was likely to have been true in at least some of the periods being considered for AT&T, see Charnes, Cooper and Schinnar (1982).

[16] See Christensen and Green (1976, p. 660).

The cost share estimates obtained from (iv) all remained in the zero-one range, as required in (i), and (ii) was also approximately satisfied in most (but not all) cases.

The condition in (vii) limits the own price elasticities to the inelastic range, $\eta_{ii} \geq -1$, but does not prevent them from becoming positive. The reasons for limiting the behavior to this range are two: (a) in the subsequent study with corrected data, as reported in Sueyoshi (1985), and Charnes, Cooper and Sueyoshi (1986), the estimates all satisfy $0 > \eta_{ii} > -1$ for each input (in every year) without the constraints (iv) and (vii) and (b) the results reported by Evans and Heckman produces similar results. See Table 2.

The constraints

$$\gamma_{ii} \leq -S_i^*(S_i^* - 1) \quad \text{(viii)}$$

would restrict own price elasticities to $\eta_{ii} \leq 0$ for the inputs used in this case but this requirement proved to be redundant. This redundancy may itself be regarded as of some importance. Evans and Heckman comment specifically on any realization of positive own price elasticities as a basis for evaluating studies by others such as Christensen (1981) which they believed were weakened, if not wholly invalidated, by the appearance of positive own price elasticities.

Others have regarded the translog function itself as being seriously flawed because it admits the possibility of positive own price elasticities and thus violates one of the "regularity requirements" of economic theory. See Barnett and Lee (1985). The idea seems to be that a suitable flexible functional form must rule out such possibilities of "*specification error.*"[17] *Economic theory, by and large*, also determines the variables to be used so that, as in (1), other variables and constraining relations needed to reflect regulatory conditions and related institutional arrangements are omitted. Statistical tests can supposedly then be used to examine whether the evidence supports the hypothesized behavior, as was noted in the discussion leading up to A and B in §3 whereas in our case (as already noted) we shall utilize sensitivity and dual evaluator analyses to study the effects of variation in data and in constraints.

6. Regression Estimates

Table 1 provides a comparison between our goal programming/constrained regression estimates and coefficients reported by Evans and Heckman. For simplicity we use only one of the two cost functions preferred by Evans and Heckman. It is the one in which they allow for first order serial correlation.

Differences are evident in the 2 sets of estimates recorded in Table 1 which include the following coefficient values that enter importantly into the "returns-to-scope and scale analyses" that are discussed in the next section.

		Coefficient Values	
Variable	Parameter	Constrained Regression	Evans and Heckman
q_2^2 = Toll²	δ_{22}	5.656	−8.018
q_1^2 = Local²	δ_{11}	4.546	−4.241
$q_1 q_2$ = Local Toll	δ_{12}	−5.204	11.663

We also provide the comparison shown in Table 2 between our estimates of own price elasticities with those reported by Evans and Heckman. It is possible, of course, to carry this further into a comparison of the "cross elasticities" that are also recorded by Evans and Heckman (1983, p. 264). They do not discuss these values, however, and so

[17] See Charnes, Cooper and Sueyoshi (1986) for alternative approaches in terms of goal programming/constrained regressions.

TABLE 1
Estimated Cost Function Coefficients

	Constrained Regression	Evans and Heckman*
Constant	9.045	9.054
Capital	0.450	0.535
Labor	0.449	0.355
Toll	−0.080	0.260
Local	0.799	0.462
Technology	−0.016	−0.193
Capital^2	0.141	0.219
Labor^2	0.113	0.174
Capital-Labor	−0.087	−0.180
Toll^2	5.656	−8.018
Local^2	4.546	−4.241
Local-Toll	−5.204	11.663
Technology^2	0.822	−0.176
Capital-Toll	0.988	0.337
Capital-Local	−0.135	−0.359
Labor-Toll	−1.131	−0.179
Labor-Local	0.141	0.164
Capital-Technology	−1.106	0.083
Labor-Technology	1.281	−0.057
Toll-Technology	−3.124	−1.404
Local-Technology	3.105	1.207

* Source: Evans and Heckman (1983, p. 260; 1984, p. 622).

we also refrain from further comment and simply note that the "own elasticities" in Table 2 are all negative, as required.

Next we provide a listing of the critical constraints as identified by the dual variables in Table 3. Focusing first on the bottom of Table 3, where results applicable to the conditions (iv) appear, we observe that the only constraints that are critical appear in years where errors in the data or aberrant behavior is noted. See Appendix Table A.1 for 1947–1948 and Figure 4, above, for the 1966–1967 aberration in the Technological index. The other constraints for this condition are not critical and hence their zero dual variable values are all lumped together in the row marked "others" at the bottom of the table.

Moving up to the next box in Table 3 we come to the conditions (vii) on own price elasticities. The lower bounds are critical only for γ_{22} and γ_{33} but the dual variable values are relatively small so that tightening the bounds set by S_2 and S_3 in (vii) would have relatively little effect on the resulting total deviations.

Similar remarks apply to the other constraints although special attention needs to be directed to the dual variables associated with the functional exhibited in (2). These dual

TABLE 2
Own Price Elasticities (1961)

	Constrained Regression	Evans and Heckman
Capital	−0.236	−0.056
Labor	−0.299	−0.151
Materials	−0.107	−0.590*

* This amount corrects for what seems to have been a decimal point error in Evans and Heckman (1983, p. 264).

TABLE 3

Binding Constraint and Dual Variable Values

Observation and Constraints		Dual Variable
Observation	1947	0.2884
	1948	−0.9094
	1951	−3.5485
	1955	−1.4545
	1957	−0.1678
	1960	−3.8156
	1963	−2.0713
	1966	0.0019
	1967	0.9257
	1968	−1.3451
	1969	−1.7240
	1972	−0.1075
	1975	−2.4257
	1976	−0.6466
	others	1.0000
Symmetry		
(Cap.-Lab.)	$\gamma_{12} = \gamma_{21}$	−0.0490
(Cap.-Mat.)	$\gamma_{13} = \gamma_{31}$	−0.0047
(Lab.-Mat.)	$\gamma_{23} = \gamma_{32}$	−0.1018
Homogeneity	$\sum_i \alpha_i = 1$	0.1235
(Capital)	$\sum_j \gamma_{1j} = 0$	−0.0344
(Labor)	$\sum_j \gamma_{2j} = 0$	0.1402
(Material)	$\sum_j \gamma_{3j} = 0$	0.0934
(Local)	$\sum_i \rho_{i1} = 0$	0.0847
(Toll)	$\sum_i \rho_{i2} = 0$	0.1682
(Technology)	$\sum_i \lambda_i = 0$	0.1387
Lower bound of γ_{ii}		
(Capital²)	$\gamma_{11} + S_1^{*2} \geq 0$	0.0000
(Labor²)	$\gamma_{22} + S_2^{*2} \geq 0$	0.0202
(Material²)	$\gamma_{33} + S_3^{*2} \geq 0$	0.0844
Price Concavity	1947 Material	0.0321
	1948 Capital	0.0156
	1948 Material	0.0405
	1966 Labor	0.0133
	1967 Material	0.0207
	others	0.0000

variable values are listed at the top of Table 3 under the heading "observation" because it is variations in observational data that are being evaluated.[18]

Help can be secured by rewriting the constraints from (2) as $\delta_t = \ln C_t - \ln \hat{C}_t$, where C_t represents the orginally observed cost for year t while $\ln \hat{C}_t = f(p_t, q_t, T_t)$ is the estimated cost for this same year.

The δ_t values are evidently expressed in logarithmic units, *viz*, $\delta_t = \ln d_t$, so that the applicable value of the dual variable is obtained via

[18] See the discussion in Charnes, Cooper and Sueyoshi (1986) and the references cited there for uses of these values as an alternative and as an aid for the usual statistical analysis of errors. See also Charnes, Cooper, Golany, Seiford and Stutz (1985, p. 100) for units invariance versions of goal programming functionals.

$$\frac{\partial \ln d_t}{\partial \ln C_t} = \frac{\partial \ln C_t}{\partial \ln C_t} = 1$$

when $\partial \ln \hat{C}_t / \partial \ln C_t = 0$, which occurs for $\delta_t^* > 0$ and the latter increases *pari passu* with increases in $\ln C_t$, as indicated by the value of the dual variable for observations classified as "others."

Turning to the negative valued dual variables for "observations" the applicable condition is $\ln C_t = \ln \hat{C}_t$. An increase in any of these observed C_t values would result in a new $f(\cdots)$ with a lower value of the total deviations, $\sum_t \delta_t$, by moving the reestimated function closer (on balance) to the other data points. The positive valued dual variables in this set have the opposite property. That is, the reestimated function needed to maintain the preceding equality would increase $\sum_t \delta_t$ by moving the function (on balance) farther from the other data points. It is therefore of interest to note that all these positive dual variables are associated with additional data errors. Reference to Table A.1, for example, shows the toll output variable, q_1, to be error in 1947 while reference to Figure 4 shows that the years 1966 and 1967 are where the aberration occurs in the technological change index.

7. Tests of Natural Monopoly

As Evans and Heckman note in their critique of Christensen (1981) and Christensen, Cummings and Schoech (1981), the concept of "economies of scope" as introduced in the more recent economics literature see Baumol, *et al.* (1982)[19]—has pointed up the need for extending the analyses of "natural monopoly" beyond testing by reference only to "economies of scale." To clarify what is involved we follow Evans and Heckman (1983, p. 133) and say that economies of scope at output levels q_1 and q_2 are present if and only if

$$C(q_1, q_2) < C(q_1, 0) + C(0, q_2). \tag{3}$$

Verbally interpreted, this means it is less costly to produce outputs q_1 and q_2 together instead of separately. Several points need to be noted as follows: First, the functional forms are the same on both sides of (3) which means that the two entities with the cost functions represented on the right are assumed to have access to the same technology as the one entity on the left. Second, all entities are assumed to use the "best" or "most economical" technologies—i.e. the efficient frontier is always achieved.

Concerning scale economies we again follow Evans and Heckman (1983, p. 282) and say that product specific scale economies[20] are present in product two if

$$\frac{C(q_1, q_2) - C(q_1, 0)}{q_2} > \frac{\partial C(q_1, q_2)}{\partial q_2}. \tag{4}$$

The term on the right represents the derivative of total cost with respect to q_2 at (q_1, q_2) while the term on the left is the average increase in total cost associated with incrementing output from 0 to q_2 while holding q_1 fixed. This implies falling average cost if the derivative is continuous over this range.

A similar development applies for q_1, but the possibility of simultaneously incrementing q_1 and q_2 is omitted from consideration, along with other possibilities like incrementing to q_2 from $q_2 - \Delta q_2$ while holding q_1 fixed. The reason for limiting the analysis in this way is not clear since the "joint cost" possibilities associated with such variations were important considerations to the economic consequences of breaking up

[19] See also Bailey and Friedlander (1982). We leave aside the problem of zero outputs in either q_1 and q_2 with logarithmic functions like (1), as discussed in Baumol *et al.* (1982) since this is not discussed in Evans and Heckman.

[20] Other more general formulations are available in Panzar and Willing (1977). See also the discussions in Banker, Charnes and Cooper (1984) and Banker (1984).

Bell. A possible reason for failure to treat this topic explicitly is that Evans and Heckman believed it was incorporated, along with economies of scale and scope, in their "natural monopoly" test which we summarize next.

The concept of natural monopoly for a multi-product firm revolves around the mathematical concept of subadditivity—*viz.*, a function $C(q_1, q_2)$ is subadditive at (q_1, q_2) if and only if

$$C(q_1, q_2) = C[\phi q_1 + (1 - \phi)q_1, \omega q_2 + (1 - \omega)q_2] \\ \leq C[\phi q_1, \omega q_2] + C[(1 - \phi)q_1, (1 - \omega)q_2] \quad (5)$$

for all $0 \leq \omega, \omega \leq 1$.

To measure subadditivity in a form that can be tested statistically Evans and Heckman develop a measure that they refer to as "$\text{Sub}_t(\phi, \omega)$," where the subscript t refers to each of the years from 1958 to 1977. The reasons for choosing these years to measure the subadditivity are discussed in Evans and Heckman (1984, p. 621 and 1983, p. 267).

We do not repeat the Evans-Heckman development here, but simply reproduce the results which are reported for each pair of ϕ and ω values used by Evans and Heckman in 1961. This is done in Table 4 where the resulting values represent estimates of gains that are possible from the indicated pairings. None of these values is statistically significant, but since they are all positive the evidence from this test indicates that the Bell System was *not* a natural monopoly, as Evans and Heckman observe. Nothing is lost and something might be gained in the way of cost savings by breaking up the entity.

Table 5 reports results from the same test applied to our constrained regression/goal programming approach. In this case the signs are all negative, so that a saving is indicated—some of them quite substantial—in every case. That is, the negative values in Table 5 represent estimated percentage losses via increased costs if Bell is broken into two separate entities producing the mixture of toll and local calls indicated by the corresponding pairs of rim values.

Of course, the results in Table 5 are not decisive. More needs to be done not only with respect to significance testing but local vs. global properties also need to be addressed. These additions to the present analysis would require substantial developments which we do not undertake because our main purpose has now been achieved. Exactly opposite conclusions may be obtained by simply changing the methods of estimation while continuing to use the same data and the same functional form. We further document

TABLE 4*

Percent Gain or Loss from Multi-Firm vs. Single Firm
Evans-Heckman (1984, *p.* 621)

ϕ \ ω	$\text{Sub}_{1961} \times 100$ (%)										
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
0.0	8										
0.1	8	8									
0.2	9	8	8								
0.3	12	10	9	9							
0.4	15	13	10	9	9						
0.5	20	16	13	11	9	9					
0.6	25	21	17	14	11	10	9				
0.7			23	18	15	12	10	9			
0.8					20	16	12	10	8		
0.9							17	13	10	8	
1.0										10	8

Note. Entries equal $\text{Sub}_{1961} \times 100$. A positive number indicates that multi-firm production is more efficient than single firm production.

TABLE 5*

Percent Gain or Loss from Multi-Firm vs. Single Firm Constrained Regression

ω \ ϕ	Sub$_{1961}$ × 100 (%)										
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
0.0	−23										
0.1	−17	−20									
0.2	−15	−16	−18								
0.3	−15	−15	−15	−16							
0.4	−18	−16	−15	−15	−15						
0.5	−24	−21	−18	−17	−15	−15					
0.6	−34	−29	−24	−21	−18	16	−15				
0.7			−34	−29	−24	−21	−18	−16			
0.8					−34	−29	−24	−20	−18		
0.9							−34	−28	−24	−20	
1.0										−28	−23

* Note: Entries equal Sub$_{1961}$ × 100. A negative number indicates that single firm production is more efficient than multi-firm production.

TABLE 6

Maximum Percent Gain From Multi-firm vs. Single-Firm Production

Year	Constrained Regression	Evans and Heckman*
1958	−11	13
59	−15	20
60	−15	25
61	−15	25
62	−16	33
63	−19	40
64	−13	44
65	−15	48
66	−20	53
67	−23	58
68	−22	51
69	−29	50
70	−33	39
71	−42	36
72	−50	39
73	−74	41
74	−86	42
75	−80	45
75	−79	59
77	−74	51

Note: Entries equal Max Sub$_t$ × 100 for each of t = 1958, · · ·, 1977.

* From Evans and Heckman (1984, p. 620).

this in Table 6, where, as may be observed, our results continue to contradict those of Evans and Heckman in every one of the pertinent years.

8. Economic and Statistical Tests

Not tested by Evans and Heckman are the following two basic assumptions: (1) the "economics assumption" of efficient production and (2) the "statistical assumption" of

multi-variate normality for the way errors in the data behave. We do not go into possible interactions between the two but only examine them separately.

Table 7 portrays observed costs under C_t and the corresponding estimates under $\hat{C}_t$ from Goal Programming/Constrained Regression (G-P/C-R) and Evans and Heckman (E-H). The large number of zeros under the G-P/C-R column is consistent with the hypothesized economic efficiency. Furthermore, as indicated by the G-P/C-R % deviations, the relative correspondences between the estimates and observed values are close with an estimated average annual absolute deviation of only 0.64%. Allowance for errors and aberrations such as the 1966 behavior of the technological change index might improve even this very good picture, but, in any event, the evidence seems remarkably consistent with the efficiency assumption.

TABLE 7

Summary of Statistical Fit

Year	Observed Cost*	G-P/C-R Constrained Regression		E-H Evans and Heckman		% Deviation	
t	C_t	$\hat{C}_t$	$C_t - \hat{C}_t$	$\hat{C}_t$	$C_t - \hat{C}_t$	G-P/C-R	E-H
1947	2550.68	2550.68	0.00	1879.40	671.28	0.00	35.72
48	2994.94	2994.94	0.00	2890.94	104.00	0.00	3.60
49	3291.06	3253.15	37.91	3115.75	175.31	1.16	5.63
50	3563.20	3556.14	7.06	3461.61	101.59	0.20	2.93
51	4047.07	4047.07	0.00	3952.11	94.96	0.00	2.40
52	4616.23	4562.08	54.15	4568.15	48.08	1.19	1.05
53	4935.13	4837.82	97.31	4891.76	43.37	2.01	0.89
54	5258.76	5129.20	129.56	5153.79	104.97	2.53	2.04
55	5770.47	5770.47	0.00	5730.41	40.06	0.00	0.70
56	6305.44	6237.66	67.78	6160.70	144.74	1.09	2.35
57	6351.19	6351.19	0.00	6307.81	43.38	0.00	0.69
58	6788.40	6689.44	98.96	6704.58	83.82	1.48	1.25
59	7334.71	7321.00	13.71	7384.25	−49.54	0.19	−0.67
60	7912.48	7912.48	0.00	8004.12	−91.64	0.00	−1.14
61	8516.46	8473.46	43.00	8552.68	−36.22	0.51	−0.42
62	9018.66	9000.99	17.67	9058.80	−40.14	0.20	−0.44
63	9508.12	9508.12	0.00	9490.07	18.05	0.00	0.19
64	10524.00	10308.88	215.12	10478.10	45.90	2.09	0.44
65	11207.00	10924.72	282.28	11026.22	180.78	2.58	1.64
66	11954.20	11954.20	0.00	12018.94	−64.74	0.00	−0.54
67	12710.90	12710.90	0.00	12584.04	126.86	0.00	1.01
68	13814.10	13814.10	0.00	13761.15	52.95	0.00	0.38
69	14940.40	14940.40	0.00	15006.80	−66.40	0.00	−0.44
70	16485.80	16284.02	201.78	16577.44	−91.64	1.24	−0.55
71	17951.80	17909.53	42.27	18492.86	−541.06	0.24	−2.93
72	20161.20	20161.20	0.00	21357.26	−1196.06	0.00	−5.60
73	21221.70	21029.61	192.09	23800.05	−2578.35	0.91	−10.83
74	23168.40	23101.55	66.85	27561.30	−4392 90	0.29	−15.94
75	27376.70	27376.70	0.00	31627.11	−4250.41	0.00	−13.44
76	31304.50	31304.50	0.00	35903.39	−4598.89	0.00	−12.81
77	36078.00	35407.15	670.85	39542.76	−3464.76	1.89	−8.76
Total Absolute Value			2,238.35		23,542.85	19.80	137.42
Average Absolute Value			72.20		759.45	0.64	4.43

* Data from Appendix Table A.1.

Evans and Heckman do not explicitly state what kind of efficiency[21] they are assuming and so we consider some of the alternate possibilities as follows: Scale efficiency was a central issue for the Bell breakup case and hence could not be assumed without invalidating the whole analysis. A distinction between technical efficiency and price or allocative efficiency might also be made in order to see whether AT&T fell short of attaining these types of efficiency. Because Evans and Heckman are silent on this topic, however, we refrain from further comment and simply observe that % deviations under the G-P/C-R column of Table 7 are all relatively small. This means that the observed values C_t are relatively close to the estimated efficiency frontier values $\hat{C}_t$ and we conclude that the behavior of the observed costs conform reasonably well to whatever combination Evans and Heckman had in mind for technical and allocative efficiencies in the years covered by these data.

The statistical assumptions of multivariate normality are another matter, as are related "regression assumptions" like (a) the absence of collinearity and (b) the absence of effects from "outliers" on their coefficient estimates. Although Evans and Heckman (1983, p. 142 ff.) provide extensive discussions of collinearity, these all take the form of criticisms directed to treatments proposed by others such as Vinod's use of "ridge regression." Nothing is explicitly said by Evans and Heckman about any methods of their own so that issues such as bias and instability in their estimates of regression coefficients are left unattended.

That such problems may be present is indicated in Table 7 by the comparison shown in the row labelled Total Absolute Value for the sums of residuals under the G-P/C-R and E-H columns. Because G-P/C-R utilizes a least absolute value measure, it is to be expected that the total of these residuals under G-P/C-R should be smaller than the amounts listed under E-H. However, a relative multiplying factor of more than 10—2,200 vs. 23,000—is much too large for what might be expected from the metrics utilized in these two different approaches in the presence of well behaved data.

The metric utilized by E-H is extremely sensitive to "outliers" whereas this is not the case for the absolute value metric used in G-P/C-R. Moreover, utilization of extreme point solution procedures such as the simplex method (Charnes, Cooper and Ferguson 1955; Charnes and Cooper 1965) eliminates the possibility of exact collinearity. Possibilities of "near collinearity," which remain, can then be detected and possibly repaired or otherwise allowed for by extensions to sensitivity analysis that are indicated in Charnes, Cooper and Sueyoshi (1986).[22]

Turning to more detailed examinations, it is evident that the behavior of the residuals for E-H is far from what would be expected from a multivariate normal distribution. Costs were always underestimated by the E-H regression from 1947 through 1958 and always overestimated from 1969 to 1977. Even more serious, from a policy-prediction standpoint, is an apparent trend toward worsening estimates in the most recent periods.

This behavior raises serious questions about the statistical estimates and tests of significance that play such a prominent role in the Evans-Heckman discussions. Although their criticism of works by others does not appear to have been responded to in kind, this may have occurred because the authors of these other studies were all members of a discipline where the checks we are using are not commonly employed—

[21] We are referring to the concepts of technical, allocative, and scale efficiency which are now common in the literature of economics following Farrell (1957), and Farrell and Fieldhouse (1962).

[22] Evans and Heckman seem to believe that collinearity is entirely a matter of the data. This is not correct. Collinearity is a problem caused by the data and/or the models used for estimation. Furthermore, a choice of solution method may also be pertinent since these methods may have mathematical properties that affect the admissible solutions, which is the reason why we have elsewhere suggested that choice of algorithms should be considered a part of the modeling process. See, e.g., the discussion of what we called "algorithmic completion of a model" in Charnes and Cooper (1965).

in which case the advisability of securing cross checks from other disciplines again becomes apparent.

The kind of "residuals examination" we have just concluded does not end the possibilities. In fields like marketing, for instance, it is common practice to use "hold-out samples" in which some of the data are reserved to test predictive power and the stability of regression coefficients that were obtained from the other (nonreserved) data. As noted earlier in this paper, Evans and Heckman did not use the data for 1978 and 1979 available from their sources—Christensen (1981) and Christensen, Christensen and Schoech (1981)—and this provides an opportunity for testing the above regressions by treating these data as if they formed a "hold-out sample."

Extrapolations from our previously estimated regressions are compared with these hold-out data in Table 8. There is some improvement in the Evans and Heckman regression—although costs continue to be overestimated. The goal programming/constrained regression, on the other hand, continues to provide a lower bound to the observed data and hence can continue to be interpreted as an efficient frontier that yields an estimated inefficiency in the vicinity of 3% for the cost performance in both of these years.

We leave aside possible uses of such inefficiency estimates, e.g., for regulatory purposes, since this would lead into a consideration of other approaches to this same topic.[23] Instead we close on a somewhat different note as follows.

Some of the hazards that might be experienced by always adhering to functions that are "everywhere smooth" when dealing with realistic data—especially when capacity limitations or other constraints are likely to be present, as in the case of AT&T and its subsidiaries during the period covered in this study—are discussed in Charnes, Cooper and Schinnar (1982). Stated differently, it might have been preferable to proceed in these studies with functions that are discontinuous in their derivatives in exchange for other properties such as ability to correct for the over- and underestimates that are apparent in the beginning and ending periods shown in Table 7.

Much is to be credited to Evans and Heckman but we have also found their accomplishments to be attended by limitations and deficiencies. These need to be taken into

TABLE 8
*Results of Extrapolations**

Year t	Observed Cost C_t	Goal Programming Constrained Regression		Evans and Heckman Regression		% Deviation	
		$\hat{C}_t$	$C_t - \hat{C}_t$	$\hat{C}_t$	$C_t - \hat{C}_t$	G-P/C-R	E-H
1978	39217.25	38177.45	1039.80	41026.56	−1809.31	2.72	−4.41
1979	44122.33	42616.64	1505.69	45095.15	−972.82	3.53	−2.16
Total Absolute Value			2545.50		2782.13	6.25	6.57
Average Absolute Value			1272.75		1391.06	3.13	3.29

* Values used for the technological change index were obtained by extrapolating the values plotted in Figure 4 to 1978 and 1979. The unusually smooth behavior from 1972 onward in Figure 4 made it seem not worthwhile to use the data that are available in Table A.4 of the Appendix in order to compute these index values more precisely.

[23] See, for instance, Thomas (1985) for a discussion of Data Envelopment Analysis as a tool for use by the Texas Public Utility Commission.

account by others who might want to go even further into the issues surrounding the breakup of the Bell System. For such persons we have included a discussion and additional tables that provide corrections to the data recorded in Table A.1 of the Appendix.

9. Summary and Conclusions

We have shown how findings from empirical studies can be altered by simply changing the method of estimation to a use of goal programming instead of the econometric methods employed by Evans and Heckman. Consistent with this alternate approach we also introduced and developed a type of dual variable analysis which provides a method that differs from the statistical analyses and error tests associated with these econometric methods. *Inter alia* this alternate approach enabled us to identify the very serious data deficiences that are discussed in the Appendix.

What about the body of concepts and theories available from microeconomics? These can be, and were, used for general guidance by us, as well as by Evans and Heckman. It needs to be recognized explicitly that there is latitude (and possible trouble) in translating these concepts for use in empirical studies. We elaborate on our choices as follows.

The frontier properties prescribed by economic theory guided the use of one-sided deviations in (2). Our purpose was to obtain a basis for examining the behavior of AT&T costs relative to the economic efficiency assumed by Evans and Heckman—and other econometricians who followed similar approaches. We knew that this would create difficulties for access to available methods of statistical hypothesis testing for which the developments to date have been fairly restricted and difficult to apply—see, for instance, the "stochastic frontier approach[24] used by Amemiya and Poirier (1976) and Aigner, Lovell and Schmidt (1977)—and the kinds of constraints we employed raised even further difficulties. In exchange for these interdictions of standard types of statistical approaches to error analysis, however, we utilized the dual to our goal programming/constrained regression model to develop another approach to the study of data variation and constraint behavior.

Given the way theories of micro-economics are formulated, ambiguities in use on empirical data almost inevitably follow. As noted in the Appendix, data deficiencies in the present case raise serious questions about the use of Shephard's lemma and related aspects of this cost and production function duality theory. Ambiguities in use would remain in any case in the use of this lemma on statistical data. For example, we concluded from Table 7 that the behavior of these cost data indicated that AT&T operated fairly close to the estimated efficiency frontier and hence conformed reasonably well to the efficiency assumptions made by Evans and Heckman. But is reasonable conformance enough? Shephard's lemma refers to the derivative of the function and approximation to a function does not guarantee that its derivatives will also be reasonably approximated. As presently formulated this theory gives almost no guidance on issues like these. This, too, is a serious deficiency when applications of this theory are to be made to empirical data. As noted in Charnes, Cooper, Golany, Seiford and Stutz (1985), what is needed is an empirically based theory, in general, and attention to the details of each application in particular. When important issues of policy are being addressed, limitations of the theory based approaches that are common in uses of

[24] This approach is built around a symmetric random error to deal with statistical behavior and a one-sided error term for managerial error. There seems to be no theory of managerial error in economics or psychology on which to ground the latter behavior, however, and the recent work by Gong and Sickles (1986) indicates serious deficiencies in these "stochastic frontier" approaches when the technologies being examined are complex.

economics on empirical data can be brought into better focus by drawing in different disciplines along lines like those indicated in this article and *vice versa*.[25]

[25] The authors are greatly indebted to A. Y. Lewin as well as to R. M. Thrall and three anonymous referees for criticisms and suggestions that resulted in considerable improvements in this paper during its several revisions. Originally prepared for presentation at the European Operational Research Society Meetings in Bologna, Italy, on June 16–19, 1985, the research for this paper was partly supported by the National Science Foundation and by USARI Contract MDA 903-83-K0312 with the Center for Cybernetic Studies at the University of Texas at Austin, Austin, Texas, USA 78712-1177. It was also partly supported by the IC[2] Institute at the University of Texas at Austin and by the Ohio State University. Reproduction in whole or in part is permitted for any purpose of the U.S. Government.

Appendix

1. *Tables*

Discussion. Table A.1 is taken from Evans and Heckman (1983, pp. 276–277), and contains the data used in this study. The parenthesized amounts indicate where errors were located in the course of our analyses and the corrected values are shown to their right. As already noted, we used only the original (uncorrected) data to obtain full comparability with results reported by Evans and Heckman (1983, 1984).

To understand the other data deficiencies in Table A.1 we note that the costs are all derived from the formulations of R. W. Shephard (1970) by means of the following identity $C_t \equiv \sum_{i=1}^{3} p_{it}q_{it}$. To state this somewhat differently, the C_t values are not separately observed but are derived from the observed values of p_{it} and q_{it} in each year $t = 1947, \ldots, 1977$. Thus from Table A.2 we have

$$C_{1967} = 6874 + 4329.1 + 1507.8 = 12710.9$$

where the cost figure on the right is labelled "Real Cost" (in \$$10^6$) with all prices stated relative to 1967 as a base period. See the row for 1967 in Table A.2 and note that all prices are set at unity in this row to give the real cost shown on the right for this year.[26]

Turning to the row for 1961 in Table A.1, it can be seen that all prices were restated relative to 1961 but the cost figures were not similarly adjusted. Thus the 1967 cost value continues to be 12710.9, as before, whereas if it had been recalculated in terms of 1961 prices, as required by the above identity, the result would have been

$$\hat{C}_{1967} = 1.04 \times 6874 + 1.3 \times 4329.1 + 1.17 \times 1507.8 = 14541$$

where "$\doteq$" means "approximately equal to."

Dividing this last expression through by the 12710.9 exhibited in the 1967 row of Table A.1 we get

$$\sum_{i=1}^{3} S_{i,1967} \doteq 0.56 + 0.44 + 0.14 = 1.14$$

where $0 \leq S_{i,1967} \leq 1$ is the cost share of input i in 1967. The "summing up to unity condition" required for the cost shares in this year is evidently not satisfied and this same result occurs for the other years covered in Table A.1.[27]

This failure to satisfy the summing up condition is to be expected, of course, since it is only another way of recognizing that the identity introduced above is not satisfied when the costs C_t, on the left, are computed relative to a 1967 price base while the expression on the right is stated relative to 1961 prices.

The fact that this identity is not satisfied also has further serious consequences since many of the estimating relations employed by Evans and Heckman are derived by means of Shephard's lemma—*viz.* $\partial C/\partial p_i = q_i$.

Evans and Heckman use this lemma repeatedly to derive estimating relations on the assumption that the observed p_{it} and q_{it} result from efficient (optimizing) behavior of the kind posited by Shephard (1970) when deriving this lemma.

[26] Although the data in Table A.2 are obtained from Christensen *et al.* (1981), the costs on the right were calculated by us. Christensen (1981) and Christensen *et al.* (1981) also do not decompose total output into its local and toll components according to the procedure described by Evans and Heckman (1983, p. 275) so we have undertaken to complete the picture in Table A.3 for convenience in use by potentially interested persons. The choice of a base period being essentially arbitrary, we elected to relate all prices to 1967 as a base as in Christensen (1981) and Christensen *et al.* (1981), rather than shifting to a 1961 base as in Evans and Heckman (1983).

[27] The capital shares and labor shares reported in the last two columns of Table A.1 are transcribed directly from Table 17 in Christensen, Christensen and Schoech (1981) and are therefore also stated relative to 1967 rather than being restated in 1961 prices as required for use by Evans and Heckman.

TABLE A.1

Bell System Data Used for Multiproduct Cost Function Estimates

Year	Cost ($\$10^6$)	Local Output	Toll Output	Capital Price	Labor Price	Material Price	R&D Index	Capital Share	Labor Share
1947	2550.68	0.41014	(0.36642) 0.34642	0.49948	0.53566	0.66952	0.57955	0.39552	0.49635
1948	2994.94	0.45783	(0.34642) 0.37201	0.55879	0.58236	0.75117	0.55445	0.40430	0.48286
1949	3291.06	0.48703	0.38296	0.57440	0.60959	0.74530	0.55261	0.41936	0.47113
1950	3563.20	0.52004	0.41592	0.61810	0.63164	0.76525	0.56980	0.44096	0.45352
1951	4047.07	0.55560	0.46552	0.70031	0.66926	0.81572	0.59576	0.45338	0.44230
1952	4616.23	0.59149	0.50116	0.79500	0.70946	0.82863	0.62057	0.46670	0.43159
1953	4935.13	0.62452	0.52271	0.80853	0.73411	0.84389	0.63873	0.46436	0.43614
1954	5258.76	0.65669	0.55000	0.81269	0.76134	0.85563	0.65059	0.46596	0.42866
1955	5770.47	0.70289	0.61941	0.86056	0.80674	0.87558	0.66162	0.47840	0.41414
1956	6305.44	0.75645	0.68394	0.88033	0.81063	0.90493	0.68018	0.47642	0.41045
1957	6351.19	0.80355	0.74006	0.81997	0.84824	0.93896	0.71436	0.47138	0.41365
1958	6788.40	0.84224	0.77663	0.87304	0.85084	0.95305	0.76830	0.50754	0.38849
1959	7334.71	0.89657	0.86274	0.91051	0.91958	0.97417	0.83934	0.52030	0.37321
1960	7912.48	0.95314	0.93512	0.95733	0.95979	0.99061	0.91902	0.53120	0.36083
1961	8516.46	1.00000	1.00000	1.00000	1.00000	1.00000	1.00000	0.54381	0.34605
1962	9018.66	1.05411	1.08231	1.01457	1.03632	1.01995	1.08533	0.55077	0.33966
1963	9508.12	1.11068	1.17451	1.00832	1.07393	1.03404	1.18984	0.55139	0.33353
1964	(10524.00) 10542.48	1.15909	1.31715	1.07804	1.12970	1.08451	1.32815	0.56240	0.32693
1965	11207.00	1.22822	1.47436	1.06139	1.17121	1.10681	1.49998	0.55286	0.32925
1966	11954.20	1.30609	1.68434	1.04475	1.22827	1.14085	1.16877	0.54302	0.33698
1967	12710.90	1.38312	1.84266	1.04058	1.29702	1.17371	1.86844	0.54079	0.34058
1968	13814.10	1.46568	2.05511	1.08325	1.36057	1.21948	2.02744	0.54614	0.33406
1969	14940.40	1.55869	2.33437	1.04579	1.49416	1.28286	2.16342	0.51402	0.35802
1970	(16485.80) 16516.87	1.63899	2.53682	1.04891	1.62387	1.35211	2.28416	0.49799	0.37133
1971	17951.80	1.70956	2.69772	1.04058	1.80415	1.42019	2.40026	0.48313	0.38304
1972	20161.20	1.80454	2.96927	1.09157	2.06226	1.47653	2.52124	0.47953	0.39061
1973	(21221.70) 21190.30	1.91210	3.31628	1.00312	2.26329	1.56221	2.65447	0.44558	0.41442
1974	23168.40	2.00785	3.60503	1.00104	2.51621	1.74061	2.80468	0.43407	0.42485
1975	27376.70	2.07532	3.86421	1.18939	2.85473	1.91315	2.97195	0.46178	0.40606
1976	31304.50	2.17307	4.24442	1.32778	3.21920	2.01408	3.15081	0.46977	0.39608
1977	(36078.00) 34745.33	2.29155	4.68449	(1.53590) 1.41935	3.40726	2.12911	3.33422	(0.48680) 0.46712	(0.37808) 0.39259

Note: Parenthesized amounts represent erroneous value for figure immediately to its right.

* Source: D. S. Evans and J. J. Heckman (1983, pp. 276–277).

TABLE A.2

*Input Quantities and Prices**

Year t	Capital		Labor		Materials		Real Cost ($\$10^6$)
	Q_t	P_t	Q_t	P_t	Q_t	P_t	
1947	2101.8	0.480	3065.5	0.413	462.7	0.596	2550.68
1948	2254.9	0.537	3220.8	0.449	528.0	0.640	2994.94
1949	2500.3	0.552	3299.0	0.470	567.5	0.635	3291.06
1950	2645.2	0.594	3318.3	0.487	576.6	0.652	3563.20
1951	2726.4	0.673	3469.1	0.516	607.4	0.695	4047.07
1952	2819.9	0.764	3642.3	0.547	665.0	0.706	4616.23
1953	2949.4	0.777	3802.9	0.566	682.9	0.719	4935.13
1954	3137.5	0.781	3840.3	0.587	760.1	0.729	5258.76
1955	3338.1	0.827	3842.1	0.622	831.2	0.746	5770.47
1956	3550.9	0.846	4141.0	0.625	925.1	0.771	6305.44
1957	3799.3	0.788	4017.1	0.654	912.7	0.800	6351.19
1958	4106.6	0.839	4020.2	0.656	869.1	0.812	6788.40
1959	4361.5	0.875	3861.0	0.709	940.9	0.830	7334.71
1960	4568.6	0.920	3858.2	0.740	1012.2	0.844	7912.48
1961	4819.3	0.961	3822.5	0.771	1100.9	0.852	8516.46
1962	5094.6	0.975	3833.9	0.799	1137.1	0.869	9018.66
1963	5410.4	0.969	3830.0	0.828	1242.0	0.881	9508.12
1964	5713.1	1.036	3950.3	0.871	1280.3	0.924	10542.48
1965	6074.5	1.020	4086.3	0.903	1400.9	0.943	11206.97
1966	6465.5	1.004	4253.8	0.947	1475.8	0.972	11954.19
1967	6874.0	1.000	4329.1	1.000	1507.8	1.000	12710.90
1968	7247.4	1.041	4399.3	1.049	1592.6	1.039	13814.12
1969	7641.6	1.005	4643.3	1.152	1748.9	1.093	14940.44
1970	8144.7	1.008	4889.6	1.252	1896.9	1.152	16516.87
1971	8673.2	1.000	4943.5	1.391	1985.3	1.210	17951.82
1972	9216.3	1.049	4953.0	1.590	2081.1	1.258	20161.19
1973	9809.3	0.964	5035.7	1.745	2214.0	1.331	21190.30
1974	10453.9	0.962	5073.8	1.940	2204.0	1.483	23168.36
1975	11060.5	1.143	5050.7	2.201	2219.6	1.630	27376.69
1976	11525.1	1.276	4983.1	2.482	2465.3	1.716	31304.54
1977	11899.0	1.364	5192.5	2.627	2687.1	1.814	34745.33

Legend Q = Quantity Index P = Price Index (using 1967 as price relatives)

* Source: L. R. Christensen, D. C. Christensen and P. E. Schoech (1981, p. 18, p. 49, and p. 52).

The validity of this lemma in relating efficient prices to efficient quantities depends critically on the validity of the preceding identity, however, and hence the limitations arising from the above noted data deficiencies need to be taken into account in the present case when interpreting results secured from the use of this lemma. See the next section of this appendix for an example of its use in deriving such estimating relations.

2. *Derivation of Condition* (iv)

The constraint (iv) in the text of this paper is derived by starting with

$$\frac{\partial C}{\partial p_i} = \frac{\partial \ln C}{\partial \ln p_i} \cdot \frac{C}{p_i} \quad \text{and}$$

$$\frac{\partial^2 C}{\partial p_i^2} = \frac{C}{p_i} \frac{\partial}{\partial p_i} \left(\frac{\partial \ln C}{\partial \ln p_i} \right) + \frac{\partial \ln C}{\partial \ln p_i} \frac{\partial}{\partial p_i} \left(\frac{C}{p_i} \right).$$

We use the translog cost function (1) in §2 of the text to define a new variable Y via

$$\frac{\partial \ln C}{\partial \ln p_i} = \alpha_i + \sum_j \gamma_{ij} \ln p_j + \sum_k \rho_{ik} \ln q_k + \lambda_i \ln T$$

and we require

$$\frac{\partial^2 C}{\partial p_i^2} = \frac{C}{p_i} \frac{\gamma_{ii}}{p_i} + Y \frac{p_i \dfrac{\partial C}{\partial p_i} - C}{p_i^2} \leq 0.$$

TABLE A.3

Bell System Data Corrected (1967 Base Period)

Year	Cost ($\$10^6$)	Local Output	Toll Output	Capital Price	Labor Price	Materials Price	R&D Index	Capital Share	Labor Share	Materials Share
1947	2550.68	0.297	0.188	0.480	0.413	0.596	0.310	0.396	0.496	0.108
1948	2994.94	0.331	0.202	0.537	0.449	0.640	0.297	0.404	0.483	0.113
1949	3291.06	0.352	0.208	0.552	0.470	0.635	0.296	0.419	0.471	0.110
1950	3563.20	0.376	0.226	0.594	0.487	0.652	0.305	0.441	0.453	0.106
1951	4047.07	0.402	0.253	0.673	0.516	0.695	0.319	0.453	0.442	0.104
1952	4616.23	0.428	0.272	0.764	0.547	0.706	0.332	0.467	0.432	0.102
1953	4935.13	0.452	0.284	0.777	0.566	0.719	0.342	0.464	0.436	0.099
1954	5258.76	0.475	0.298	0.781	0.587	0.729	0.348	0.466	0.428	0.105
1955	5770.47	0.508	0.336	0.827	0.622	0.746	0.354	0.478	0.414	0.108
1956	6305.44	0.547	0.371	0.846	0.625	0.771	0.364	0.477	0.410	0.113
1957	6351.19	0.581	0.402	0.788	0.654	0.800	0.382	0.471	0.414	0.115
1958	6788.40	0.609	0.421	0.839	0.656	0.812	0.411	0.508	0.388	0.104
1959	7334.71	0.648	0.468	0.875	0.709	0.830	0.449	0.520	0.373	0.106
1960	7912.48	0.689	0.507	0.920	0.740	0.844	0.492	0.531	0.361	0.108
1961	8516.46	0.723	0.543	0.961	0.771	0.852	0.535	0.544	0.346	0.110
1962	9018.66	0.762	0.587	0.975	0.799	0.869	0.581	0.551	0.340	0.110
1963	9508.12	0.803	0.637	0.969	0.828	0.881	0.637	0.551	0.333	0.115
1964	10542.48	0.838	0.715	1.036	0.871	0.924	0.711	0.562	0.326	0.112
1965	11206.97	0.888	0.800	1.020	0.903	0.943	0.803	0.553	0.329	0.118
1966	11954.19	0.944	0.914	1.004	0.947	0.972	0.893	0.543	0.337	0.120
1967	12710.90	1.000	1.000	1.000	1.000	1.000	1.000	0.541	0.341	0.119
1968	13814.12	1.060	1.115	1.041	1.049	1.039	1.085	0.546	0.334	0.120
1969	14940.44	1.127	1.267	1.005	1.152	1.093	1.158	0.514	0.358	0.128
1970	16515.87	1.185	1.377	1.008	1.252	1.152	1.222	0.497	0.371	0.132
1971	17951.82	1.236	1.464	1.000	1.391	1.210	1.285	0.483	0.383	0.134
1972	20161.19	1.305	1.611	1.049	1.590	1.258	1.349	0.479	0.391	0.130
1973	21190.30	1.382	1.800	0.964	1.745	1.331	1.421	0.446	0.415	0.139
1974	23168.36	1.452	1.956	0.962	1.940	1.483	1.501	0.434	0.425	0.141
1975	27376.69	1.500	2.097	1.143	2.201	1.630	1.591	0.462	0.406	0.132
1976	31304.54	1.571	2.303	1.276	2.482	1.716	1.686	0.470	0.395	0.135
1977	34745.33	1.657	2.542	1.364	2.627	1.814	1.784	0.467	0.393	0.140

TABLE A.4

*Bell System Data Corrected** (1967 *Base Period*)

Year	Cost $10^6	Local** Output	Toll** Output	Capital Price	Labor Price	Materials Price	R&D Price	R&D Quantity	Capital Share	Labor Share	Materials Share
1978	39217.25	1.797	3.109	1.475	2.832	1.950	196.4	2.211	0.463	0.391	0.146
1979	44122.33	1.902	3.501	1.546	3.120	2.108	248.2	2.093	0.449	0.399	0.152

* Adopted from Christensen's testimony Christensen (1981) and Christensen *et al.* (1981).

** We computed these values using the data in Christensen (1981) and Christensen *et al.* (1981) based on the Tornqvist index procedure described in Evans and Heckman (1983, p. 275).

By Shephard's lemma, $\partial C/\partial p_i = x_i$, where x_i is the ith optimal input at price vector p. Hence it is possible to relate this concavity condition to own price elasticities when $p_i, x_i > 0$ since then

$$\frac{p_i}{x_i}\frac{\partial^2 C}{\partial^2 p_i} = \frac{p_i}{x_i}\frac{\partial x_i}{\partial p_i} = \eta_{ii} \leq 0$$

when $\partial^2 C/\partial p_i^2 \leq 0$.

As noted in the preceding section of this Appendix, data difficulties in this application make it questionable to use Shephard's lemma in this fashion. Hence it is best to regard this relation to own price elasticities as holding only loosely, and work with the expression preceding this last derivation instead. Since $p_i^2 > 0$ the expression on the right for $\partial^2 C/\partial p_i^2$ can be replaced by

$$\gamma_{ii}C + Y\left[p_i\frac{\partial C}{\partial p_i} - C\right] \leq 0.$$

For any $C > 0$, this last expression can be changed to

$$\gamma_{ii} + Y\left[\frac{\partial \ln C}{\partial \ln p_i} - 1\right] \leq 0 \qquad \text{or} \qquad \gamma_{ii} + Y^2 - Y \leq 0.$$

Equivalently we then have $Y - \gamma_{ii} \geq Y^2$ which we weaken to $Y - \gamma_{ii} \geq 0$. Direct substitution and collection of terms produces the conditions displayed in (iv) of the text for each $i = 1, 2, 3$, *viz.*

$$\alpha_i + \sum_j \gamma_{ij} \ln p_j + \sum_k \rho_{ik} \ln q_k + \lambda_i \ln T \geq \gamma_{ii}$$

which, it may be noted, does not restrict γ_{ii} to nonpositive range.

Bibliography

AIGNER, D. J., T. AMEMIYA AND D. J. POIRIER, "On the Estimation of Production Frontiers," *Internat. Economic Rev.*, 17 (1976), 377–396.

——— AND S. F. CHU, "On Estimating the Industry Production Function," *Amer. Economic Rev.*, 58 (1968), 826–839.

———, K. LOVELL AND P. SCHMIDT, "Formulation and Estimation of Stochastic Frontier Production Function Models," *J. Economics*, 6 (1977), 21–37.

ARMSTRONG, R. D. AND M. T. KUNG, "An Algorithm for a Least Absolute Value Regression Problem with Bounds on the Parameters," *App. Math. and Computation*, 7 (1980), 267–279.

——— AND ———, "A Dual Algorithm to Solve Linear Least Absolute Value Problems," *J. Oper. Res. Soc.*, 33 (1982), 931–936.

BAILEY, E. E. AND A. E. FRIEDLAENDER, "Market Structure and Multiproduct Industries," *J. Economic Lit.*, 20 (1982), 1024–1041.

BANKER, R., "Estimating Most Productive Scale Size Using Data Envelopment Analysis," *European J. Oper. Res.*, 17 (1984), 35–44.

———, A. CHARNES AND W. W. COOPER, "Models for Estimating Technical and Scale Efficiencies in Data Envelopment Analysis," *Management Sci.*, 30 (1984), 1078–1092.

———, ———, ——— AND A. MAINDIRATTA, "A Comparison of DEA and Translog Estimates of Production Frontiers Using Simulated Observations from a Known Technology," in A. Dogrammaci and R. Färe (Eds.), *Applications of Modern Production Theory: Efficiency and Productivity*, Kluwer Academic Publishers, Boston, 1987.

BARNETT, W. A. AND Y. W. LEE, "The Global Properties of the Miniflex Laurent, Generalized Leontief, and Translog Flexible Functional Forms," *Econometrica*, 53 (1985), 1421–1437.

BARRODALE, I. AND F. D. K. ROBERTS, "An Efficient Algorithm for Discrete L_1 Linear Approximation with Linear Constraints," *SIAM J. Numerical Anal.*, 15 (1978), 603–611.

BASSETT, G. J. AND R. KOENKER, "Asymptotic Theory of Least Absolute Error Regression," *J. Amer. Statist. Assoc.*, 73 (1978), 618–622.

BAUMOL, W. J., J. C. PANZAR AND R. D. WILLIG, *Contestable Markets and the Theory of Industry Structure*, Harcourt Brace Jovanovich, Inc., New York, 1982.

BEWLEY, R., *Allocation Models*, Ballinger, Inc., Cambridge, Mass., 1986.

CAVES, D. W., L. R. CHRISTENSEN AND J. A. SWANSON, "Productivity Growth Scale Economies, and Capacity Utilization in U.S. Railroads," *Amer. Economic Rev.*, 71 (1981), 994–1002.

CHARNES, A. AND W. W. COOPER, "A Strategy for Making Models in Linear Programming," Ch. 26 in R. Machol et al. (eds.), *System Engineering Handbook*, McGraw-Hill Book Co., Inc., New York, 1965.

———, ——— AND R. O. FERGUSON, "Optimal Estimation of Executive Compensation by Linear Programming," *Management Sci.*, 1 (1955), 138–151.

———, ———, B. GOLANY, L. SEIFORD AND J. STUTZ, "Foundations of Data Envelopment Analysis for Pareto-Koopmans Efficient Empirical Production Functions," *J. Econometrics*, 30 (1985), 1–17.

———, ——— AND F. RHODES, "Measuring Efficiency of Decision Making Units," *European J. Oper. Res.*, 3 (1979), 339–359. See also 15 (1984), 333–334.

———, ——— AND A. SCHINNAR, "A Theorem on Homogeneous Functions and Extended Cobb-Douglas Forms," *Proc. Nat. Acad. Sci.*, 73 (1976), 3747–3748.

———, ——— AND ———, "Transforms and Approximations in Cost and Production Function Relations," *Omega*, 10 (1982), 207–211.

———, ———, L. SEIFORD AND J. STUTZ, "Invariant Multiplicative Efficiency and Piecewise Cobb-Douglas Envelopments," *Oper. Res. Lett.*, 2 (1983), 101–103.

———, ——— AND T. SUEYOSHI, "A Goal Programming/Constrained Regression Analysis of AT&T as a Natural Monopoly," in O. A. Davis (Ed.), *Cost/Benefit Analysis* (to appear).

———, ——— AND ———, "Least Squares/Ridge Regressions and Goal Programming/Constrained Regression Alternatives," *European J. Oper. Res.*, 27 (1986), 146–157.

CHRISTENSEN, L. R., "Testimony of L. R. Christensen," United States v. AT&T, C.A. No. 74-1698, Defendant's Exhibit D-T-100, 1981.

———, D. C. CHRISTENSEN AND P. E. SCHOECH, "Total Factor Productivity in the Bell System 1947–1979," Christensen Associates Inc., 810 University Bay Dr., Madison, WI 53705, 1981.

———, D. CUMMINGS AND P. E. SCHOECH, "Econometric Estimation of Scale Economies in Telecommunications," Working Paper 8124 SSRI University of Wisconsin, Madison, 1981. Subsequently published (1983) in *Economic Analysis of Telecommunications: Theory and Applications*, L. Courville, A. de Fontenay and R. Dobell (eds.), Elsevier Science, Amsterdam, 1981.

——— AND W. H. GREENE, "Economies of Scale in U.S. Electric Power Generation," *J. Political Economy*, 84 (1976), 655–676.

———, D. W. JORGENSEN AND L. J. LAU, "Transcendental Logarithmic Production Frontiers," *Rev. Economics and Statist.*, 55 (1973), 28–45.

EVANS, D. S. AND J. J. HECKMAN, "Multiproduct Cost Function Estimates and Natural Monopoly Tests for the Bell System," Ch. 10 in D. S. Evans (Ed.), *Breaking Up Bell*, Elsevier Science Publishers, Amsterdam, 1983.

——— AND ———, "Natural Monopoly," Ch. 6 in D. S. Evans (Ed.), *Breaking Up Bell*, Elsevier Science Publishers, Amsterdam, 1983.

——— AND ———, "Test for Subadditivity of the Cost Function with an Application to the Bell System," *Amer. Economic Rev.*, 74 (1984), 615–623.

FARRELL, M. J., "The Measurement of Productive Efficiency," *J. Roy. Statist. Soc. Ser.* A, 120, part 3, 157 (1957), 253–281.

——— AND M. FIELDHOUSE, "Estimating Efficient Production Frontiers Under Increasing Returns to Scale," *J. Roy. Statist. Soc. Ser.* A, (1962), 252–267.

GONG, BYEONG-HO AND R. SICKLES, "Finite Sample Evidence on the Performance of Frontier Models Using Panel Data," Rice University, Houston, TX, Department of Economics, October 1986.

GUILKEY D. AND C. LOVELL, "On the Flexibility of the Translog Approximation," *Internat. Economic Rev.*, 16 (1980), 137–147.

HANOCH, G. AND M. ROTHSCHILD, "Testing the Assumptions of Production Theory: A Nonparametric Approach," *J. Political Economy*, 80 (1972), 256–275.

KOENKER, R. AND G. J. BASSETT, "Regression Quantiles," *Econometrica*, 46 (1978), 33–50.

MCFADDEN, D., "Cost, Revenue and Profit Functions," in M. Fuss and D. McFadden (Eds.), *Production Economics: A Dual Approach to Theory and Applications*, North Holland, Amsterdam, 1978.

MEYER, J. R., R. W. WILSON, M. A. BAUGHCUM, E. BURTON AND L. CAOUETTE, *The Economics of Competition in the Telecommunications Industry*, Oelgeschlager, Gunn & Hain, Publishers, Cambridge, Mass., 1982.

PANZAR, J. C. AND R. D. WILLIG, "Economies of Scale in Multi-Output Production," *Quart. J. Economics*, (1977), 481–493.

PHILLIPS, P. C. B., "The Exact Distribution of the SUR Estimator," *Econometrica*, (1985), 745–756.

RAJ, B. AND H. VINOD, "Bell System Scale Economies Estimated from a Random Coefficient Model," *J. Economics and Business*, 34 (1982), 247–252.

REDER, M. W., "Chicago Economics: Permanence and Change," *J. Economic Lit.*, 20 (1982), 1–38.

SHEPHARD, R. W., *Theory of Cost and Production Functions*, Princeton University Press, Princeton, N.J., 1970.

SHERMAN, D., "Measurement of Hospital Technical Efficiency: A Comparative Evaluation of DEA and Other Approaches," Harvard University Graduate School of Business DBA Thesis, 1981.

SUEYOSHI, T., "Goal Programming/Constrained Regression and Alternative Approaches to Statistical Estimation," Unpublished Ph.D. Thesis, University of Texas, Austin, Graduate School of Business, 1986.

THOMAS, D., "Using Data Envelopment Analysis to Evaluate The Performance of Electric Utility Cooperatives in Texas," Unpublished Ph.D. Thesis, University of Texas, Austin, Graduate School of Business, 1985.

U.S. GENERAL ACCOUNTING OFFICE, "Legislative and Regulatory Action Needed to Deal with a Changing Domestic Telecommunications Industry," CED 81-136, Washington, D.C., 1981.

VARIAN, H. R., *Microeconomic Analysis*, 2nd ed., W. W. Norton, Inc., New York, 1984.

VINOD, H. D., "Applications of New Ridge Regression Methods to a Study of Bell System Scale Economies," *J. Amer. Statist. Assoc.*, 71 (1976), 835–841.

——, "Nonhomogeneous Production Functions and Applications to Telecommunications," *Bell J. Economics and Management Sci.*, 3 (1972), 531–543.

——, "Poisson Model for Measuring the Returns to Research and Development in Public Utilities," in O. D. Anderson (Ed.), *Forecasting Public Utilities*, North-Holland, Amsterdam, 1980.

ZELLNER, A., "An Efficient Method of Estimating Seemingly Unrelated Regression Equations and Tests for Aggregation Bias," *J. Amer. Statist. Assoc.*, (1968), 348–368.

STATEMENT BY THE EDITOR-IN-CHIEF

The two previous articles address a number of issues involving the breakup of the Bell System which are also of general scientific importance. Additional very important issues have been raised during the reviewing of these two articles. However, the review process of *Management Science* cannot resolve most of these additional concerns or questions. Interested readers may contact either or both sets of authors for additional information, comments or currently unpublished working papers.

MANAGEMENT SCIENCE
Vol. 34, No. 1, January 1988
Printed in U.S.A.

REJOINDER

NATURAL MONOPOLY AND THE BELL SYSTEM: RESPONSE TO CHARNES, COOPER AND SUEYOSHI

DAVID S. EVANS AND JAMES J. HECKMAN
Department of Economics, Fordham University, Bronx, New York 10458
Department of Economics, University of Chicago, Chicago, Illinois 60637

Our study of the Bell System cost function shows that it is possible to reject the hypothesis that AT&T was a natural monopoly. Our study is based on a regression analysis of a translog cost function estimated on 1947–1977 data. Charnes, Cooper, and Sueyoshi (henceforth CCS) claim that they reverse our conclusion when they use goal programming estimators of a translog cost function estimated on exactly the same data that we use. This claim is false. There is no basis for comparing our estimates with their estimates because they, in fact, use different data than we use and estimate a different cost function than we estimate. Moreover, when goal programming estimates and regression estimates based on the same data and similar cost function specifications are compared, they yield similar estimates and produce the same inference about natural monopoly.

CCS also assert that we make serious data errors which invalidate our results. They are especially concerned about our use of costs measured using a 1967 base year and prices measured using a 1961 base year as regressors. While there are a few errors in our published data, most are typographical errors that were not made in processing our data; correction of the few remaining errors has little effect on our parameter estimates or test for natural monopoly. (See Evans and Heckman 1986 for details.) The difference in base years has *no* effect on the natural monopoly tests. They falsely accuse us of using erroneous cost share data. They then go on to produce estimates based on erroneous share data of their own creation.

In this reply, we make seven main points. We do not reply to numerous more minor misrepresentations of our work in the CCS paper.

(1) The CCS claim that we measure costs in 1967 dollars (factor prices in 1967 normalized to unity) and use factor prices denominated in 1961 dollars (factor prices in 1961 normalized to unity) as regressors is correct. Such a change in base prices is innocuous in a translog model since it results in an *exact* reparameterization of the cost function. Estimates of the cost function based on the reparameterized cost function *must* produce forecasts of costs and cost shares identical to those obtained from a parameterization using 1967 denominated factor prices as regressors. Tests of homogeneity, symmetry and cross-equation restrictions are unaffected by this reparameterization. *No change in any important inference can result from using* 1961 *base prices.*

The CCS claim that the cost shares used in our empirical work are erroneous and do not sum to one is false. Our reported cost share data are identical to the data they (correctly) claim are appropriate. *Their generation and use of erroneous cost share data injects a new element into the analysis of the Bell System data that has not appeared in any of our studies.*

(2) The CCS comparison between goal programming and regression procedures is not informative because it compares estimates of two different models based on different data. CCS impose different constraints than we do and use erroneous cost share data to impose constraints while we use the correct cost share data. We control for serial

0025-1909/88/3401/0027$01.25

correlation in the data and they do not. When comparable models are fit using alternative goal programming and regression methodologies, there is little difference in the estimates of the technology or the inference about natural monopoly. *The real lesson to be learned from CCS and this reply is that it is the constraints imposed on the technology and not the method of estimation that matters.*

(3) Goal programming as formulated by CCS is unsuitable for conducting statistical inference. This is unfortunate in the light of the crucial role of constraints in generating the estimates and the growing importance of statistical evidence in public policy analysis and in the courts.

(4) The CCS comparison of models in terms of goodness of fit and properties of errors is misleading. When similar models are compared, there is no evidence supporting goal programming procedures over regression procedures.

(5) CCS misinterpret our test for natural monopoly. It is a test of necessary conditions. Rejection of necessary conditions (as we have found) is informative. Acceptance (which they claim to find) is ambiguous. The only way to prove that AT&T was a natural monopoly is to determine the cost function for *all possible* output configurations, not just a subset of possible outputs.

(6) The Bell system data are fragile. They are not informative unless they are "supplemented" with prior information. The real problem with interpreting these data is deciding what types of prior information should be imposed and not whether they should be imposed via regression analysis or goal programming.

(7) CCS do not properly state the objective of our study which was to rebut evidence in support of natural monopoly set forth by AT&T experts in U.S. vs AT&T. AT&T claimed that it was a natural monopoly and hence had the burden of proof to establish that it was a natural monopoly. Using the AT&T experts' translog regression framework generalized to allow for multiple outputs we demonstrated that the data were not consistent with the hypothesis that AT&T was a natural monopoly. Relaxing further assumptions until the technology is imprecisely determined simply reinforces our point. The evidence does not support the AT&T claim in any statistically meaningful sense nor have CCS documented that they do so.

(1) The Unimportance of the Alleged Data "Errors"

CCS claim that, in the notation of their equation (1), we measure p_i with error because different base year prices are used for the calculation of p_i than are used in the calculation of C. In fact we scale p_i by a time invariant w_i that is the ratio of 1967 prices for factor i relative to 1961 prices. We use $\ln z_i = \ln p_i + \ln w_i$ in translog equation (1) in place of $\ln p_i$. The prices used as *regressors* in our analysis are transformed in this fashion. The transformation has no effect on our tests for symmetry, homogeneity and natural monopoly.

In their Appendix A, CCS go on to assert incorrectly that our cost share data are measured with error. The cost share data used in our study are based on 1967 price weights. Table 1 presents the capital share data and labor share data used in our previous study. (See Table 10.14 of Evans and Heckman 1983, p. 277.) The shares reported in Table 1 are in *exact* agreement with the shares implicit in the "correct" Table A.2 of CCS (except for a known error in 1977 shares already noted in Evans-Heckman 1986.) We do *not* use 1961 denominated factor prices to compute cost shares as CCS claim we do. Therefore our data do *not* violate the "summing up" condition that costs shares must sum to unity. A central premise of the CCS paper is false.

What are the consequences of the *transformation* in base prices that we *did* make? Trivial algebra using $\ln p_i = \ln z_i - \ln w_i$ in CCS equation (1) reveals that

TABLE 1
Cost Share Data

	Cost Shares in Correct Data (CCS Table A.2)		Cost Shares Used in Our 1983 Study**	
Year	Capital Share	Labor Share	Capital Share	Labor Share
1947	0.39552	0.49635	0.39552	0.49635
1948	0.40430	0.48286	0.40430	0.48286
1949	0.41936	0.47113	0.41936	0.47113
1950	0.44096	0.45352	0.44096	0.45352
1951	0.45338	0.44230	0.45338	0.44230
1952	0.46670	0.43159	0.46670	0.43159
1953	0.46436	0.43614	0.46436	0.43614
1954	0.46596	0.42866	0.46596	0.42866
1955	0.47840	0.41414	0.47840	0.41414
1956	0.47642	0.41045	0.47642	0.41045
1957	0.47138	0.41365	0.47138	0.41365
1958	0.50754	0.38849	0.50754	0.38849
1959	0.52030	0.37321	0.52030	0.37321
1960	0.53120	0.36083	0.53120	0.36083
1961	0.54381	0.34605	0.54381	0.34605
1962	0.55077	0.33966	0.55077	0.33966
1963	0.55139	0.33353	0.55139	0.33353
1964	0.56240	0.32693	0.56240	0.32693
1965	0.55286	0.32925	0.55286	0.32925
1966	0.54302	0.33698	0.54302	0.33698
1967	0.54079	0.34058	0.54079	0.34058
1968	0.54614	0.33406	0.54614	0.33406
1969	0.51402	0.35802	0.51402	0.35802
1970	0.49799	0.37133	0.49799	0.37133
1971	0.48313	0.38304	0.48313	0.38304
1972	0.47953	0.39061	0.47953	0.39061
1973	0.44558	0.41442	0.44558	0.41442
1974	0.43406	0.42485	0.43406	0.42485
1975	0.46178	0.40606	0.46178	0.40606
1976	0.46977	0.39508	0.46977	0.39508
1977	0.46712	0.39259	0.48680*	0.37808*

* This discrepancy is noted and its minor impact is assessed in Evans and Heckman (1986).
** Source: Evans and Heckman (1983, Table 10.14 p. 277).

$$\begin{aligned}\ln C = \alpha_0^* &+ \sum_i \alpha_i^* \ln z_i + \sum_k \beta_k \ln q_k + \mu^* \ln t + \tfrac{1}{2} \sum_i \sum_j \gamma_{ij} \ln z_i \ln z_j \\ &+ \sum_i \sum_k \rho_{ik} \ln z_i \ln q_k + \sum_i \lambda_i \ln z_i \ln t + \tfrac{1}{2} \sum_k \sum_l \delta_{kl} \ln q_k \ln q_l \\ &+ \sum_k \theta_k \ln q_k \ln t + \tau (\ln t)^2 \quad \text{where} \end{aligned} \qquad (1)'$$

$$\alpha_0^* = \alpha_0 - \sum_i \alpha_i \ln w_i + \tfrac{1}{2} \sum_i \sum_j \ln w_i \ln w_j,$$

$$\beta_k^* = \beta_k - \sum_l \rho_{lk} \ln w_l, \qquad \mu^* = \mu - \sum_l \lambda_l \ln w_l,$$

$$\alpha_i^* = \alpha_i - \sum_j (\ln w_j)\left(\frac{(\gamma_{ij} + \gamma_{ji})}{2}\right).$$

If the technology is symmetric, $(\gamma_{ij} = \gamma_{ji})$ and $(\gamma_{ij} + \gamma_{ji})/2 = \gamma_{ij}$. (CCS assume sym-

metry.) Equation (1)′ is an *exact* reparameterization of (1). The subadditivity test reported in our previous papers can validly be based on either (1) or (1)′ provided that the appropriate price (p_i or z_i) is used.

Observe that estimates of (1)′ recover γ_{ij}, ρ_{ik}, λ_i, δ_{kl}, θ_k and τ in exactly the same form as they appear in (1). The cost share equation from (1)′ corresponding to equation (iv) in CCS is

$$S_i = \frac{\partial \ln C}{\partial \ln z_i} = \alpha_i^* + \sum_l \left(\frac{\gamma_{il} + \gamma_{li}}{2}\right) \ln z_l + \sum_k \rho_{ik} \ln q_k + \lambda_i \ln t. \tag{2}$$

It is valid to test *cross equation* restrictions linking (1)′ and (2). The test for symmetry (condition B in CCS) is unaffected. The test for homogeneity (condition A is CCS) is also unaffected. To see this note that the tests $\sum \lambda_i = 0$, $\sum_j ((\gamma_{ij} + \gamma_{ji})/2) = 0$, $\sum_j \rho_{jk} = 0$ are obviously unaffected by the reparameterization. The same is true for $\sum \alpha_i = 1$ since $\sum_i \alpha_i^* = \sum_i \alpha_i$ because $\sum_i \sum_j ((\gamma_{ij} + \gamma_{ji})/2) \ln w_j = 0$. [Observe that $\sum_i \sum_j ((\gamma_{ij} + \gamma_{ji})/2) \ln w_j = \sum_j \ln w_j \sum_i ((\gamma_{ij} + \gamma_{ji})/2) = 0$ since $(\gamma_{ij} + \gamma_{ji})/2$ is symmetric in i and j.][1] *Our inferences about symmetry, homogeneity and natural monopoly are unaffected by using* 1961 *base prices rather than* 1967 *prices. Our "error" is an innocuous reparameterization of technology.*

CCS also claim that there are other data errors in our work. Some of these are typographical errors in our tables that do not effect our computations. The remaining errors are all minor and have no effect on the empirical results as is evident from comparing in columns 1 and 2 of Tables 2 and 3 discussed below.

(2) A False Comparison Is Made Between Goal Programming and Regression Procedures

For a comparison between goal programming and regression methods to be of interest, the same model should be estimated. Yet the model estimated by CCS differs substantially from our specification. First, we do not impose the inequality constraints (iv) or (vii) that they do. Constraint is natural. (vii) forces the price elasticities to be inelastic. Even if these constraints were valid they are not interesting ones to impose when the share data are erroneous. CCS create erroneous share data by measuring total costs in 1967 denominated factor prices and individual input costs in 1961 dollars. Second, we use the information that cost shares sum to one ($\sum S_i = 1$). This is a valid condition for the cost share data we use. It is not valid for the erroneous data generated by CCS. They thus use different cost share data than we do and they do not use the information on cost shares available to us. Third, they do not correct for serial correlation in the data despite its time trended nature (see Figures 1–4 in their paper) and despite the fact that it is unlikely that efficiency errors—the sole source of the disturbance term in the CCS model—are uncorrelated over time. We test for serial correlation, find it to be important and correct for its effects. They ignore serial correlation in forming all of their estimates.

Because different data are used, different constraints are imposed, and a different treatment of serial correlation is given, the comparison offered by CCS is uninformative. Fortunately, in other work CCS (1986) use the correct cost share data and impose

[1] Only our statistical tests for separability ($\rho_{il}\beta_k = \rho_{ik}\beta_l$) and nonjointness ($\delta_{kl} = -\beta_k\beta_l$, $k \neq l$) are affected by the reparameterization. In empirical results available on request, we find that both hypotheses are rejected (as they were in our 1983 paper) in a model with no serial correlation correction and that separability is *not* rejected but nonjointness is when a serial correlation correction is made. Estimates of our model that do not impose separability still reject the natural monopoly hypothesis.

TABLE 2

Comparison of Estimates of Alternative Models (Standard errors in parentheses)

	Translog Regression Models			Goal Programming Models		New Translog Regression Models	
	1[a]	2	3[b]	4	5	6	7
Constant	9.054	9.054	9.450	9.443	9.045	9.052	9.04198
	(0.005)	(0.005)	(0.007)			(0.0047)	(0.005237)
Capital	0.535	0.536	0.543	0.513	0.450	0.6616	0.41504
	(0.008)	(0.008)	(0.009)			(0.1389)	(0.12436)
Labor	0.355	0.354	0.341	0.[illegible]66	0.449	0.5535	0.0099
	(0.007)	(0.007)	(0.007)			(0.2594)	(0.2364)
Local	0.462	0.206	1.196	0.466	0.799	−0.2080	−0.06699
	(0.226)	(0.299)	(0.392)			(0.3650)	(0.28465)
Toll	0.260	0.504	−0.100	0.348	−0.080	0.3316	0.40631
	(0.309)	(0.219)	(0.266)			(0.3220)	(0.26057)
Technology	−0.193	−0.201	−0.070	−0.190	−0.016	0.1983	0.20637
	(0.086)	(0.086)	(0.150)			(0.2009)	(0.16012)
Capital^2	0.219	0.223	0.223	0.171	0.141	0.3570	−0.27031
	(0.024)	(0.026)	(0.026)			(1.5921)	(0.99301)
Labor^2	0.174	0.174	0.174	0.175	0.113	3.530	2.8748
	(0.027)	(0.027)	(0.027)			(6.9231)	(5.343)
Capital-Labor	−0.180	−0.183	−0.183	−0.139	−0.087	1.2281	−0.69635
	(0.019)	(0.020)	(0.020)			(1.4675)	(1.31025)
Local^2	−16.360	−16.646	−16.642	−12.707	4.546	−2.0951	−10.262
	(4.340)	(4.310)	(4.305)			(15.7621)	(12.165)
Toll^2	−8.482	−8.969	−8.970	−5.030	5.666	10.5717	5.519
	(2.628)	(2.595)	(2.595)			(9.3325)	(6.556)
Local-Toll	11.663	12.167	12.167	8.588	−5.204	−3.7467	−10.265
	(3.144)	(3.105)	(3.105)			(12.2613)	(12.165)
Technology^2	−0.176	−0.180	−0.180	0.290	0.822	2.2557	−0.16507
	(1.033)	(0.517)	(0.517)			(1.0471)	(0.966826)
Capital-Local	0.337	0.343	0.343	0.200	−0.135	1.0720	−0.4689
	(0.138)	(0.140)	(0.140)			(3.8799)	(2.720)
Labor-Local	−0.359	−0.362	−0.362	−0.162	0.141	2.5936	1.1318
	(0.122)	(0.123)	(0.123)			(4.7684)	(3.256)
Capital-Toll	−0.179	−0.180	−0.180	0.003	0.988	−0.0885	1.604
	(0.083)	(0.085)	(0.085)			(3.3987)	(2.386)
Labor-Toll	0.164	0.161	0.161	0.043	−[illegible].131	−6.7646	−4.113
	(0.071)	(0.072)	(0.072)			(4.0471)	(3.371)
Capital-Technology	0.083	0.081	0.081	−0.077	−1.106	−1.2047	−1.4834
	(0.053)	(0.054)	(0.054)			(1.03712)	(0.6472)
Labor-Technology	−0.057	−0.052	−0.052	0.076	1.281	5.6563	3.310
	(0.047)	(0.048)	(0.048)			(1.90612)	(1.488)
Local-Technology	−1.404	−1.553	−1.555	−0.466	3.105	4.71603	8.2287
	(1.497)	(1.503)	(1.503)			(3.0438)	(2.424)
Toll-Technology	1.207	1.430	1.431	−0.368	−3.124	−7.9398	−5.606
	(1.431)	(1.431)	(1.431)			(3.4080)	(2.364)
Autocorrelation	—	—	—	—	—	—	0.31
							(0.046)

TABLE 2 (*cont'd*)

	Translog Regression Models			Goal Programming Models		New Translog Regression Models	
	1[a]	2	3[b]	4	5	6	7
Percent Shares							
Cost	0.187 (0.105)	0.186 (0.105)	0.186 (0.105)	—	—	—	—
Shares	0.712 (0.094)	0.706 (0.093)	0.706 (0.093)	—	—	—	—

[1] Original estimates corrected for misprints—Evans and Heckman (1983).
[2] Estimates corrected for minor data errors but with base year discrepancy—Evans and Heckman (1986).
[3] Estimates corrected for typographical errors and using 1967 base year for prices—these are new estimates.
[4] Goal programming estimates on corrected data with summing-up constraint—Charnes, Cooper, and Sueyoshi (1986).
[5] Goal programming estimates without summing-up constraint on uncorrected data—Charnes, Cooper, and Sueyoshi, this issue.
[6] Regression estimates without summing-up constraint—these are new estimates not controlling for serial correlation.
[7] Same as 6 except controlling for serial correlation.
[a] There are some labelling and other errors in the estimates reported in Evans and Heckman (1983b, 1984), we have corrected these in this table. See Evans and Heckman (1986) for details.
[b] For consistency we normalize toll, local and technology to unity in 1967.

the summing up condition that we use although they do not control for serial correlation in the errors. Comparability is enhanced because they do not impose inequality conditions (iv) and (vii) of their current paper. A more informative comparison is one between the estimates reported in their other paper and the estimates of our model, although even this comparison ignores the different treatment of serial correlation.

Table 2 reports estimates of the parameters of (1)′ and (1). Columns (1)–(3) are based on our work. Column (2) is our model as discussed by CCS. A comparison between columns 1 and 2 demonstrates the negligible effect of the data errors on our estimates. The regression model reported in column 3 uses 1967 as a base for factor prices and corrects for the minor errors noted in our 1986 paper. Column 4 reports the estimates in CCS (1986). Column 5 records the estimates given by CCS in this issue. Columns 6 and 7 record new regression estimates of translog cost functions obtained by minimizing *only* the deviations between predicted and actual log cost. The estimator used to obtain the numbers in 6 does *not* control for serial correlation while the estimator for 7 does.

A more legitimate comparison than the one offered by CCS is one between the translog estimates reported in column 3 and the goal programming estimates reported in column 4. Both models use the correct cost share data and both use the adding up condition. Both use the 1967 base year prices. The agreement in the estimates between columns 3 and 4 is rather close. The product-moment correlation between comparable coefficients in the two equations is 0.9834. The local^2, toll^2 and local-toll coefficients that are crucial to our natural monopoly test are similar. Regression estimates that do not adjust for serial correlation are even closer to the CCS estimates.[2]

Table 3 reports the outcome of natural monopoly tests described in CCS for all seven models. A comparison of columns 2 and 3 provides a numerical proof of the trivial

[2] These results are available on request from the authors.

algebraic fact that cost functions (1) or (1)' provide exactly the *same* inference on the natural monopoly question. A comparison of columns 3 and 4 reveals that the CCS model most comparable to our 1983 model gives very similar results on the natural monopoly test.

TABLE 3
Comparison of Natural Monopoly Tests Based on Alternative Estimates (Standard errors in parentheses)
Maximum Percent Gain From a Two-Firm Breakup

	Model[a]						
Year	1	2	3	4	5	6	7
1958	13	12	12	−14	−11	−48	52
	(15)	(15)	(15)			(35)	(16)
1959	20	17	17	−10	−15	−69	51
	(14)	(14)	(14)			(44)	(18)
1960	25	19	19	−7	−15	−85	51
	(14)	(14)	(14)			(55)	(19)
1961	25	22	22	0	−15	−105	52
	(14)	(14)	(14)			(72)	(21)
1962	33	27	27	1	−16	−119	53
	(14)	(13)	(13)			(83)	(21)
1963	40	32	32	7	−19	−141	54
	(15)	(13)	(13)			(99)	(22)
1964	44	40	40	22	−13	−139	65
	(15)	(13)	(13)			(100)	(19)
1965	48	46	46	28	−15	−124	70
	(16)	(16)	(16)			(155)	(18)
1966	53	51	51	17	−20	−173	74
	(23)	(18)	(18)			(102)	(7)
1967	58	55	55	18	−23	−211	75
	(23)	(20)	(20)			(127)	(17)
1968	51	56	56	16	−22	−214	77
	(26)	(22)	(22)			(130)	(16)
1969	50	54	54	19	−29	−245	76
	(30)	(25)	(25)			(138)	(18)
1970	39	55	55	20	−33	−277	74
	(21)	(26)	(26)			(154)	(19)
1971	36	52	52	21	−42	−349	70
	(21)	(26)	(26)			(191)	(22)
1972	39	47	47	21	−50	−443	65
	(21)	(28)	(28)			(255)	(27)
1973	41	43	43	23	−74	−509	61
	(20)	(20)	(10)			(299)	(31)
1974	42	44	44	23	−86	−535	59
	(21)	(20)	(10)			(322)	(33)
1975	45	47	47	26	−80	−553	63
	(20)	(20)	(20)			(345)	(32)
1976	59	50	50	31	−79	−625	63
	(20)	(19)	(19)			(419)	(33)
1977	51	52	52	35	−74	−576	67
	(19)	(19)	(10)			(404)	(31)

[a] Column numbers correspond to column numbers in Table 2.

In order to compare regression estimates with the CCS estimates reported in this journal, translog estimates reported in column 6 of Table 2 are derived from minimizing the squared discrepancy between predicted and actual log cost. The estimator used to generate the estimates does not control for serial correlation and does not impose the summing up constraint and is most comparable to the CCS estimator reported in this journal (reported in column 5 of Table 2). Note the similarity in the estimates and the results of the natural monopoly test. There is less comparability in this case because CCS impose constraints (iv) and (vii) which we do not and they use erroneous share data to impose their constraints. Notice how imprecisely determined are the column 6 estimates and note the ambiguity in the natural monopoly test (the max value is never significantly different from zero in a statistical sense).

The message from these tables is clear. It is the constraints imposed on the problem and not goal programming or regression methodology that creates the differences in estimates of production technology parameters.

Note further that controlling for serial correlation in the errors makes a great deal of difference in the natural monopoly test. Column 7 of Table 3 is based on a translog regression analog of CCS model 5 reported in this journal. Unlike the model of column 6, the estimates reported in column 7 are obtained from a procedure that corrects for first order serial correlation. Notice how the inference from models 5 and 6 is reversed. *Using a regression model similar to the CCS model reported in this journal, but controlling for serial correlation, we find statistically significant evidence against the hypothesis that AT&T was a natural monopoly in every year of our sample.*

(3) Goal Programming (*As Formulated*) Is Unsuitable for Conducting Statistical Inference

Given the crucial role of the constraints in generating the differences in the estimates that we have just documented, it is unfortunate that goal programming as formulated by CCS cannot be used to test for the validity of the constraints. In place of rigorous statistical inference, CCS are forced to make statistically unsupported assertions about the size of scale dependent multipliers (or dual variables), the importance of critical hypotheses about parameter restrictions and the significance of gains or losses from breaking up AT&T. Since their estimator has no known sampling distribution, readers of their work have no objective, scientific way to evaluate whether their multiplier variables are statistically significantly different from zero and whether their estimates of break up gains or cost function parameters are statistically significantly different from zero or our numbers unless there are literally no errors in the data or variations in firm efficiency. This limitation of goal programming is important because the use of statistical significance concepts has become widespread in public policy analysis and in the courts. It is ironic that in faulting us for imposing false constraints on the data, CCS are using statistical inferences about these constraints obtained from translog regression methodology that cannot be made using goal programming.

(4) The CCS Comparison of Models in Terms of Goodness of Fit and Properties of the Errors Is Misleading

The estimates of our model reported in columns 1–3 of Table 1 are chosen to minimize a quadratic form in deviations between predicted and actual log costs and cost shares. We choose parameters to minimize

$$(\ln C - \ln \hat{C}, S - \hat{S})'Q(\ln C - \ln \hat{C}, S - \hat{S}) \tag{3}$$

where ln C is the sample vector of log costs, S is a matrix of sample cost shares, " ^ " denotes predicted value and Q is a positive definite matrix. CCS use a metric of

summed nonnegative deviations from log costs (see their equation (2)). Because they minimize in a metric that only considers fit of the log cost equation and we use a metric that considers the fit of the log cost and share equations, comparisons of the "fit" of their model as reported in their Table 7 that evaluate model adequacy for predicting costs is biased toward goal programming. We fit *three* time series of length 31 with 21 parameters. They fit only one of the three time series with 21 parameters. The degrees of freedom left in their time series are considerably less than in ours.

A fairer comparison is one between the CCS model (column 5 in Table 1) and the translog model 6 in Table 1. Both models are fit to log cost data alone (albeit using different metrics) and neither imposes the summing up constraint or corrects for serial correlation in the data. Table 4 presents evidence on the predictive accuracy of model (6) for cost data in the format of Table 7 in CCS which reports the fit of model 5 and model 2. Note that model 6 has a lower total absolute value deviation (1507.3) and mean absolute deviation (48.6) than does CCS model 5 (2,238.3 and 72.2 respectively). Another fair comparison is between CCS model 4 which uses the summing up condi-

TABLE 4
Actual and Predicted Values of Costs from Model of Column 6

Year	C	$\hat{C}$
1947	2550.7	2549.2
1948	2994.9	2989.8
1949	3291.1	3290.7
1950	3563.2	3577.6
1951	4047.1	4055.7
1952	4616.2	4631.0
1953	4935.1	4937.2
1954	5258.8	5212.1
1955	5770.5	5762.7
1956	6305.4	6305.6
1957	6351.2	6357.6
1958	6788.4	6758.1
1959	7334.7	7352.2
1960	7912.5	7933.3
1961	8516.5	8544.0
1962	9018.7	9038.0
1963	9508.1	9516.1
1964	10524.5	10524.3
1965	11207.0	11126.3
1966	11954.2	11905.7
1967	12710.9	12799.1
1968	13814.1	13756.6
1969	14940.4	15153.4
1970	16485.5	16362.8
1971	17951.8	17885.3
1972	20161.0	20240.5
1973	21221.7	21019.6
1974	23168.4	23259.3
1975	27376.7	27386.5
1976	31304.5	31440.8
1977	36078.0	35998.1
R^2:		0.99992
Total Absolute Value Deviation		1507.2975
Average Absolute Value Deviation		48.6225

TABLE 5
Summary Fit Measures for Cost and Share Data

	CCS (Model 4)				Translog (Model 3)			
	Cost	Capital Share	Labor Share	Material Share	Cost	Capital Share	Labor Share	Material Share
R^2	0.99969	0.78384	0.81460	0.89490	0.99980	0.91938	0.91538	0.87926
Mean Absolute Deviation of Residuals	96.10431	0.01357	0.01456	0.00367	81.34396	0.01113	0.01139	0.00360
Total Absolute Deviation of Residuals	2979.234	0.42067	0.45136	0.11377	2521.66	0.34503	0.35309	0.1116

tion and translog model 3 where this condition is also imposed. Table 5 presents such a comparison. The translog estimates are slightly better in all dimensions (except R^2 for material shares) than are the goal programming estimates. The main message of Tables 4 and 5 is that *the constraints imposed and not the fitting methodology drive the results.*

CCS claim that we need multivariate normality for our errors. It is well known that the SUR estimation method that we use is robust to nonnormal errors (see, *e.g.* Amemiya 1985). The correlated nature of the unadjusted residuals from model 2 does not indicate a failure of that model. Precisely because of such serial correlation patterns, we control for serial correlation in our estimation. The *adjusted* residuals from our model display a random pattern as measured by the Durbin-Watson test statistic.

(5) CCS Misinterpret Our Test for Natural Monopoly

Our test is one of necessary conditions for natural monopoly. Rejection of that hypothesis is informative. Acceptance within a region as occurs for the models that do not impose the summing up condition and that do not control for serial correlation (models 5 and 6) is not informative. All that acceptance demonstrates is that there are inefficient ways to break up AT&T. Failure to reject the necessary conditions for natural monopoly does not imply that AT&T was a natural monopoly. A test for natural monopoly requires (in the CCS notation) that Max $\mathrm{Sub}_i(\phi, \omega)$ be negative for *all* possible output configurations. To perform such a test requires extrapolation of the estimated cost function well outside the range of the data used to estimate it. Moreover, even if such extrapolation were valid, CCS could not test the hypothesis of natural monopoly statistically since the sampling distribution is unknown for the natural monopoly test statistic based on goal programming estimates.

(6) The Fragile Nature of the Evidence

The only clear lessons that emerge from Tables 2 and 3 are that constraints imposed on the data and corrections for serial correlations critically affect the estimates. This is not surprising in light of the length of the time series on costs (31 observations), the highly trended nature of the data and in light of the fact that 21 parameters are being estimated *after* homogeneity and symmetry are imposed onto the model. If these restrictions are not imposed the model has 32 parameters. Even if cost share data are used in conjunction with the cost data, there are few degrees of freedom in the data. Sharp inference can be obtained from such limited information only if prior information is imposed.

The restrictions we imposed—symmetry and homogeneity of degree one in prices—seem reasonable. The former is a second order differentiability assumption and the latter only asserts that if prices of inputs double the cost of producing with a fixed set of

inputs doubles. Our restrictions are more intuitively plausible (conventional?) than CCS restrictions (iv) and (vii) which are, respectively, *necessary* conditions for concavity of the cost function in prices and a restriction that input elasticities be inelastic. Restrictions based on erroneous share data do not seem useful.

The key point to extract from this discussion, however, is the conditional nature of inference that is possible from the Bell data. The data are not informative unless they are "supplemented" with prior information, a lamentably common state of affairs with economic time series data. The real issue, unresolved by CCS, is the issue of which types of prior information should be used to explore the natural monopoly question and not which metric to use in forming constrained estimates since constraints can be imposed in either estimation scheme. The only sharp conclusion that emerges from the empirical evidence is the importance of controlling for serially correlated errors.

(7) The Objectives of Our Study

Apart from the need to impose prior restrictions onto the time trended Bell System data to make any sharp inference about the cost function, there was another reason to impose the restrictions that we used. As government rebuttal witnesses in U.S. vs. AT&T, we necessarily examined the evidence set forth by AT&T in support of their claim to be a natural monopoly. AT&T had the burden of proof to establish their claim. To do so, they presented evidence on natural monopoly using a translog, single output, cost function estimated by regression methods that assumed that the vast array of services produced by that company could be summarized by a single index.

Following the only sensible rule in doing comparative empirical work—to relax one assumption at time—we tested and relaxed a substantively important assumption that a single output measure summarized AT&T technology. Maintaining all of the other assumptions imposed on the data by the AT&T experts, including symmetry and homogeneity, we found that there is little evidence for natural monopoly within a multiple output translog production framework. Relaxing further assumptions until the estimated technology becomes imprecisely estimated (see the contrast in the precision with which models 3 and 6 are determined) could not help the AT&T natural monopoly case. Establishing that the data are consistent with the presence or absence of natural monopoly as CCS do (model 5) or as is apparent in model 6, is not useful evidence that AT&T was a natural monopoly. It is evidence that the unrestricted data are uninformative on the matter.

Conclusion

Goal programming and regression analyses of similar models produce similar parameter estimates, measures of goodness of fit and inference about natural monopoly. The finding that AT&T is not a natural monopoly depends on the treatment of serial correlation and the nature of the constraints imposed. Without constraints imposed, the existing Bell System data are not informative on the natural monopoly issue. Finding better data and not alternative estimation metrics would be a more fruitful line of inquiry.[3]

[3] We are grateful to Bo Honore for valuable comments.

References

AMEMIYA, T., *Advanced Econometrics*, Harvard University Press, Boston, 1985.

CHARNES, A., W. W. COOPER AND T. SUEYOSHI, "A Goal Programming/Constrained Regression Analysis of AT&T as a Natural Monopoly," unpublished, January 1986, (derived from Chapter 5 of T. Sueyoshi,

"Goal Programming/Constrained Regression and Alternative Approaches To Statistical Estimation," unpublished Ph.D. Thesis, University of Texas, Austin, 1985).
———, ——— AND ———, "A Goal Programming/Constrained Regression Review of the Bell System Breakup," *Management Sci.*, 34 (January 1988).
CHRISTENSEN, L., D. CUMMINGS AND P. SCHOECH, "Econometric Estimation of Scale Economics in Telecommunications," SSRI Working Paper 8124, Social Systems Research Institute, University of Wisconsin, Madison, 1981.
EVANS, D. S. AND J. J. HECKMAN, "Natural Monopoly," in D. S. Evans (Ed.), *Breaking Up Bell*, North Holland, New York, 1983a, 127–156.
——— AND ———, "Multiproduct Cost Function Estimates and Natural Monopoly Tests for the Bell System," in D. S. Evans (Ed.), *Breaking Up Bell*, North Holland, New York, 1983b, 253–282.
——— AND ———, "A Test for Subadditivity of the Cost Function with an Application to the Bell System," *Amer. Economic Rev.*, 74 (September 1984), 615–623.
——— AND ———, "Erratum," *Amer. Economic Rev.*, 76 (September 1986), 856–858.

STATEMENT BY THE EDITOR-IN-CHIEF

The two previous articles address a number of issues involving the breakup of the Bell System which are also of general scientific importance. Additional very important issues have been raised during the reviewing of these two articles. However, the review process of *Management Science* cannot resolve most of these additional concerns or questions. Interested readers may contact either or both sets of authors for additional information, comments or currently unpublished working papers.

CCS Report 588

A Preface and a Post Script to Evans and Heckman Studies for the Bell System Breakup

by

A. Charnes
W. W. Cooper
T. Sueyoshi

February, 1988

BACKGROUND

The statement by the Editor-in-Chief which appears at the end of the preceding article provides us with an opportunity to make additional material available which we prepared in the course of the exchanges that developed as a result of our review of whether the studies by Professors Evans and Heckman (and others) showed that AT&T was not a "natural monopoly." We are pleased by the opportunity provided by the statement by Donald Morrison, as Editor-in-Chief of Management Science, since we believe that allowance for fully open criticism and responses plays an important role in acquiring and disseminating scientific knowledge and that the editorial process should encourage such criticisms and responses while ensuring that they are accurate and clearly stated. We only regret that we were prevented from making parts of these criticism more generally available since we believe that this additional material could further scientific progress by helping others to take up some of the issues with possibly different points of view and greater success.

This note serves as a preface to some of this additional material. It also serves as a postscript to the preceding two articles on the AT&T breakup which we have reproduced here for ease of reference. We also comment on the editorial processes and finally offer some suggestions for improvement in these processes more generally.

PREFACE TO EXHIBITS I AND II

EXHIBIT I which is appended to this note was written for Management Science on the invitation of Donald Morrison, who first welcomed and then rejected it on the basis of protests by Professors Evans and Heckman. We can appreciate and even symphathize with Dr. Morrison, who was periodically submitted to pressures from Professors Evans and Heckman in their efforts to prevent publication of our article. Nevertheless, we believe that Management Science should have also published Exhibit I as a useful road map to warn potentially interested readers of the errors to be found in the writings of Evans and Heckman. Without such guidance persons who intend to do research on these topics may find themselves needlessly involved in months of hard work and frustration -- such as we experienced, until we discovered that the source of our difficulties lay in the errors in the Evans and Heckman publications.

It is perhaps of interest to see what can be encountered in the way of frustration and difficulty when using the data as published by Evans and Heckman (1983). Given the way the costs were supposedly derived -- as described in the discussion of (1) ff. in Exhibit II, below -- it was clear that statistical variations should not have caused any trouble since statistical errors in the dependent variable, when present, would have had to be caused by the same errors occuring in the independent variables. We, therefore, searched for the source of our "no-solution" difficulties in "typos" and/or computational errors that also occurred as recorded in Table A.1 of our article as reproduced above. Eliminating these errors, however, still left us with only a "no solution" situation. Extended experiments with variations in our models were also undertaken to ensure that we were not

misunderstanding parts of the presentations in Evans and Heckman. These efforts also failed to resolve the "no-solution" difficulties we were encountering. We were therefore forced to look for other possible causes for our troubles and this finally drove us back to the original sources of the data in Christensen, Christensen and Schoech (1981). Extending the data from the latter source by computations such as those shown in the last column of our Table A.2, we ultimately determined that the main source of our troubles lay in the fact that every single data point was wrong for the data published, as reported by Evans and Heckman (1983) and reproduced in Table A.1 of the Appendix to our article.

Although they did not succeed in preventing the publication of our article in Management Science, Evans and Heckman did manage to secure a substantial delay. While this was occurring, they were allowed to submit and publish their Erratum, as referenced in Exhibit II, despite the fact that we wrote to Orley Ashenfelter, the editor of the American Economic Review, to inform him that ample opportunities were being provided to Evans and Heckman for effecting any needed comments and corrections in Management Science. As a result of Ashenfelter's rejection of this advice still further errors were admitted into the literature and, having allowed these additional errors into the literature by its editorial processes, the AER is now apparently willing to let them remain unattended.

After taking nearly a year to reach his decision on the 8-page (double-spaced) note we had submitted to AER, Ashenfelter, in his capacity as Editor, transmitted to us the following one-sentence report from his referee: "The Evans and Heckman 1986 Erratum [published in the AER] made mention of this data problem, asserting [sic] that it was minor, so that it may be preferable to publish nothing." All of Evans and Heckman's estimates and assertions were thus accepted

by Ashenfelter, apparently without question, even though our note reported that we had not been able to reproduce these estimates with either corrected or uncorrected data, and with and without allowance for serial correlation. Also unattended by the AER is any consideration of the erroneous statement by Evans and Heckman that the thus-noted errors in the data would affect only the intercept constant. See Exhibit II.

In the response that we have reproduced from Management Science, Evans and Heckman now assert that their data errors are "innocuous" because they result in an "exact reparameterization" in the functional form they employ. We shall shortly show that this assertion is also not correct. First, however, we need to note that nothing is said by Evans and Heckman concerning the use of these data, possibly by others, for different purposes. Second, we want to record our belief that the addition to the errors effected by the Evans and Heckman rejoinder in this issue of Management Science is a further result of the editorial processes used. We also believe that we should not have been required to delete our criticisms of certain "tendencies" in economics and in defense of this we note that publication of these kinds of criticisms (of dominant points of view which are influential in whole disciplines) have been important to the development of modern science since at least the time of Galileo, Descartes.and Pascal.

POSTSCRIPT TO THE REJOINDER BY EVANS AND HECKMAN

We begin our discussion of the preceding rejoinder by Evans and Heckman by turning first to the claim in their opening paragraph that we use different data than they do. Possibly because of where this claim is positioned, it gives an impression that Evans and Heckman are saying now that they used data that differ

from the data they originally published and cited as the source of their estimates in Evans and Heckman (1983 and 1984). Any such disclaimer in this form, however, is simply unacceptable now for publication in the scientific literature since (a) it is known that their estimates as well as their data, as published, had been challenged as in our submission to the AER which is reproduced in Exhibit II, below, and (b) no alternate data have been provided, either by citation or otherwise. We therefore conclude that the possibility of such an inference is an inadvertant consequence of where their remark is positioned and that this is not the meaning intended by Evans and Heckman--except for their claims about differences in the cost-share data which we comment on below.

Using terms like "alleged data errors" -- cf., the heading of their section (1) --Evans and Heckman seek to absolve themselves of blame for the errors they are themselves responsible for introducing into their published data. Their rationale is that their use of a translog cost function produces "an exact reparametrization" which supposedly implies that their tests on sub-additivity and hence their tests of the evidence for natural monopoly are unaffected.

Generally speaking, any reparameterization needs to be checked since it may yield statistical estimates and results that differ from those obtainable from the original (unreparameterized) model. See pp. 24-25 in G. Judge, W. Griffith, R. Hill, H. Lutkepohl and T. C. Lee, The Theory and Practice of Econometrics New York: John Wiley, 1985. By "exact reparameterization", however, Evans and Heckman seem to mean that when this is achieved the "estimates of the cost function based on the reparameterized cost function must [their italics] produce forecasts of costs and cost shares identical to those obtained from [the original] parameterization using 1967 denominated factor prices as regressors." Cf. the first of the seven main points listed on the opening page of their Rejoinder.

To show that their claim is not correct, it suffices to note that their formulation (1)' is erroneous in that the first condition they list is incorrect. It should instead appear as

$$\alpha^*_0 = \alpha_0 - \sum_i \alpha_i \ln w_i + 1/2 \sum_i \sum_j \gamma_{ij} \ln w_i \ln w_j$$

Written in this form, it is evident that the γ_{ij} are involved in α_0^*. Alternatively, moving these terms into the functional will affect the γ_{ij} estimates. In either case, these alterations will generally affect the estimates of other parameters as well and thus the "exact reparameterization" with the properties claimed by Evans and Heckman is not achieved. Q.E.D.

A logical next task would be for us to try to reproduce the estimates that Evans and Heckman have published in their rejoinder. This would require using both the corrected and uncorrected versions of their model to see what was actually done. However, we believe that this task is best left to others since, in the course of an effort that occupied nearly a whole summer, we have already failed to reproduce results reported by Evans and Heckman from earlier models. See Exhibit II below, and the references cited therein. Thus we refrain from undertaking this task again because we do not anticipate that our performance will be improved in the present (less certain) context of our understanding of what Evans and Heckman did, or should have done.

In the second paragraph of (1) in their Rejoinder, Evans and Heckman assert

that we generated new cost share data and thus introduced a new element into the analysis of the Bell System data. They then go on to assert that the comparison between goal programming and regression is not informative because the comparisons between the two are based on different data sets.

None of these assertions is correct. The cost share data we used, as taken from Christensen's court testimony are the same as the data used by Evans and Heckman. See Christensen, Christensen and Schoech (1981) p. 54, Table 17, and compare Exhibits A3 and A1 in the appendix to our article. As explained on page 19 of our article, our "no solution" troubles arose because the cost and cost shares data as published in Evans and Heckman (1983) were denominated in 1967 prices while the other data were denominated in 1961 prices.[1]

There is little point in reproducing the discussions on the developments associated with (i) on pp. 8 ff in our article. We only need note that (iv) is the expression used by Evans and Heckman in their original papers -- see, e.g., expression (10.19) on p. 258 of Evans and Heckman (1983) --while (2) is nowhere mentioned prior to its appearance here in their rejoinder.

In section (6) of their rejoinder, Evans and Heckman conclude that the only clear lessons that emerge from comparing their results with ours is that constraints imposed on the data and corrections for serial correlations critically affect the estimates. An attempt to respond to the corrections for serial correlation will only

[1] It is for this reason that we effected the additional computations needed to place all data on the same basis, as is done in Table A3 to make it possible for others to proceed without undue trouble -- as we ourselves did in the paper we contributed to the volume (to appear) that is being edited by O. A. Davis. See also Sueyoshi, 1986.

carry us back to the issue of what data were used since the deviations recorded in our Table 7 were obtained from the data of Table A.1 applied to the Evans and Heckman model which corrected for serial correlation -- as reported in Evans and Heckman (1983) and Evans and Heckman (1984).

Our choice of the title "Constrained Regression" was intended to explicitly recognize the importance of the constraints. The constraints we used in the preceding article, however, were intended only to reproduce, as closely as possible, the conditions assumed by Evans and Heckman in conformance with the requirements of the economic theory of production they were using. Perhaps reflecting differences in the disciplines being relied upon, our own preference would have oriented us differently in a manner that would have prompted us to use constraints (and data) that reflected conditions under which AT&T was operating[1] -- especially after rejecting important parts of the underlying economic theory, as was done by Evans and Heckman. See the opening paragraph in section 3 of our article.

Proceeding in the latter manner might not have been as responsive as Evans and Heckman were to the formulations of other economists. Nevertheless, it is our view that differences like these in modeling approaches as well as estimation methodologies can be brought to the fore with advantage in cases like the one we are considering. Finally, we note that it was our methodology that uncovered the errors in the data published by Evans and Heckman when we repeatedly received reports of "no solution" while using data which, by construction, had to conform to

[1]See also our discussion in Charnes, Cooper and Schinnar (1976) of deficiencies in the Shepard formulations of Cost and Production Function relations and in the use of translog estimates obtained from data which reflect the presence of capacity limitations.

the conditions we were imposing. This discovery of data troubles also turned out to be an advantage that could be secured from our use of an alternate methodology where, again, we note that a use of such alternatives does not preclude uses of the statistical methods employed by Evans and Heckman (in common with other economists).

We could extend this discussion in order to deal with still other points of interest. We prefer instead to regard this note as either a postscript or a preface to the preceding and following material, to which readers can now refer, and to the extent that we are able to do so we will be glad to respond to further questions and requests from persons who want to pursue these matters in still further detail.

ALTERATION AND EXPERIMENTATION IN EDITORIAL PROCESSES

In conclusion, we turn to editorial processes more generally and try to move from criticism to constructive suggestions. Scientific knowledge is supposed to be <u>publicly available</u> to all who are willling to learn what is required to evaluate its assertions and its accompanying processes of acquisition and validation. This includes full disclosure and accountability which extends to the processes of publication and dissemination, we think. <u>In camera</u> procedings such as are now associated with refereeing and other parts of commonly used editorial processes have never fit very well with these requirements of publicly available knowledge. To make matters worse, some journals have now extended this to a use of "double-blind" refereeing in which the names of the authors of an article are not disclosed to the referees and the names of the referees are not disclosed to the authors.

This has led to ultra-conservative and sometimes even irresponsible refereeing as well as loss of information to all concerned. The process is most likely to show its defects when new directions of research are being opened with the risks of error that seem almost always to be present in the writings associated with such endeavors. We therefore suggest that the time has come to experiment with new ways of approaching the refereeing and editorial processes which will be more in harmony with the science requirements of publicly available knowledge and accountability.

One direction that such experimentation could take is to open up the editorial process in an exactly opposite direction to the one in which double-blind refereeing is moving. Especially when new directions of research are involved, we would like to suggest publication of the referee reports (in suitably modified form) over the signatures of the referees with opportunity for a published response by the authors. We believe this will provide more information for readers, improve the refereeing process and accelerate the progress of science, including its validation.

This is not the place to argue the issues at length and so we stop only with this one suggestion: Experiment! Some boldness will be needed, of course, but the following couplet from Goethe may help:

"Whatever you can do,
Or dream you can,
Begin it;
Boldness has genius, power
and magic in it."

The alternative of inaction will more than likely only encourage present tendencies and lead still further away from the requirement of public knowledge (including knowledge of the processes used) in acquiring,disseminating and challenging scientific information. In the words of Casey Stengel, the former manager of the New York Yankees, "They say you can't do it, but sometimes you can't always not do it that way."

EXHIBIT I

A GUIDED TOUR OF A FAMILY OF STUDIES ON THE AT&T BREAKUP

(Submitted to Management Science and rejected)

by

A. Charnes
W. W. Cooper
T. Sueyoshi

May, 1987

Within any limitations of space that might reasonably be allowed, it is not possible to respond in adequate detail to the preceding critique by Drs. Evans and Heckman. We therefore forego such a rebuttal and proceed, instead, on an alternative course in which we supply specific references to the already abundant publications that have flowed from the study by Evans and Heckman (1983), which was originally commissioned by the U.S. Justice Department for use in their successful attempt to break up AT&T. We also supply comments for guidance to persons interested in pursuing one or more of the issues

To start, we reference the very long, carefully worded and crafted critique by Evans and Heckman which immediately precedes this note. This critique is directed, for the most part, to our article in this same issue of Management Science, which is also very long and, we hope, carefully worded and crafted. Readers are therefore advised to examine what is said in this exchange with more than the usual amount of care.

Particular attention is called to the material in Part One of the Appendix to our article where the errors in the Evans and Heckman 1983 publication, both typographical and otherwise, were first uncovered[1/] by virtue of the "no solution" responses in computer printouts. The correct data as obtained from Christensen, Christensen and Schoech (1981) and Christensen, Cummings and Schoech (1981) are also included in this same Part One of the Appendix so interested readers can conduct their own tests with these data as well as the other data reported there.

1/ As taken from T. Sueyoshi (1985). Unless otherwise noted, all citations are to the references listed at the end of our article in the preceding pages.

No discussion of any of these errors appears in either of the original publications of Evans and Heckman (1983, 1984), but the errors covered in the Appendix to our article do not end the matter. Readers should also refer to the nearly three pages of errata and corrigenda published by Evans and Heckman (1986) in The American Economic Review which is cited in their critique of our article. In this same issue of the AER, there is also a two-page critique by Sueyoshi and Anselmo (1986a) on the uses and interpretations of the natural monopoly test by Evans and Heckman. This critique, in turn, is a condensation of the original ms. which is available from Sueyoshi and Anselmo (1986b) for persons interesting in examining the even more severe criticisms of the Evans-Heckman natural monopoly test which Sueyoshi and Anselmo had to omit from the final publication.

Even this does not end the matter, since we also prepared an eight page criticism of the Evans and Heckman (1986) note in which (a) we pointed to additional errors that had not been included in the errata they listed and (b) we went on to delineate additional errors introduced in Evans and Heckman (1986) while they were attempting to correct the errors contained in their original (1983, 1984) publications. We still do not know whether our critique will be published by the AER and so we list it below and will respond to requests for copies, at least until the issue of publication is decided. Readers of our critique will find that we were unable to reproduce the estimates reported by Evans and Heckman even when using their methods on the "adjusted", and the "unadjusted" and the "corrected" or "uncorrected "data and with and without adjustments for serial correlation. A fuller account

may be found in the report by Sueyoshi and Anderson (1986) with an accompanying analytical development by Anderson (1986).[2/] Finally, reference may be made to the study by Charnes, Cooper and Sueyoshi which is to appear in a book on Cost/Benefit Analysis being edited by O.A. Davis (to appear) where still different results are reported. See references at the end of our article. Here, too, we are willing to supply copies of this report pending its promised publication.

Persons interested in pushing deeper into issues involved in the AT&T breakup should refer to the important series of articles by Christensen (1981), Christensen, Christensen and Schoech (1981) and Christensen, Cummings and Schoech (1983). All of us, Evans and Heckman as well as we, must be indebted to these authors for their very extensive collection and treatment of AT&T data as reported by Christensen, Christensen and Schoech (1983).

To provide still further guidance to readers interested in undertaking their own analyses, we need to say that we hope that they will be able to obtain more positive responses than we were able to secure in our requests for information from Evans and Heckman.

We would feel remiss if we concluded without making explicit acknowledgement to the numerous referees who were involved in the repeated rounds of reviews and revisions of our ms. We are especially indebted to

2/ Sueyoshi, T. and R.G. Anderson, "Results of Seemingly Unrelated Regression Applied to the Bell System Data" (August, 1986). The report as well as Anderson's (December, 1986) are available from T. Sueyoshi, The Ohio State University, College of Business, School of Public Administration, 1775 College Road, Columbus, Ohio, 43210.

the Editors, Arie Lewin and Donald Morrison, who carried the process still further and who were required to undertake extraordinary efforts and to display unusual care and patience in our case and, we believe, in dealing with the efforts by Evans and Heckman as well. Now that this long process is nearing its end, we hope that Drs. Lewin and Morrison will find that their efforts are rewarded when still other persons begin to examine what has been said, perhaps in new ways and with new points of view. We know that this will provide us with a feeling of satisfaction and we hope that this will also be true for Drs. Evans and Heckman when subsequent developments show that we have all contributed to resolving, or at least uncovering, serious issues in science and public policy formation -- no matter what these additional studies may show.

REFERENCES

Charnes, A., W.W. Cooper, T. Sueyosni, "On Errors in the Evans and Heckman Bell System Breakup Studies," American Economic Review, (submitted, October 1986).

Sueyoshi, T. and P.C. Anselmo, "The Evans and Heckman Subadditivity Test: Comment," American Economic Review, 76, 4 (1986a), 854-855.

Sueyoshi, T. and P.C. Anselmo, "Operational Difficulty of the Evans and Heckman Subadditivity Test," Working Paper Series WPS 86-134, College of Business, The Ohio State University (1986b).

EXHIBIT II

(Submitted to the American Economic Review and rejected)

On Errors in the Evans and Heckman Bell System Breakup Studies

by

A. Charnes
W. W. Cooper
and
T. Sueyoshi*

October, 1986

*Ohio State University

The note by Evans and Heckman [9] recites a whole litany of errors they committed in their U.S. Justice Department commissioned study -- which purported to show that AT&T was not a natural monopoly and hence could be broken up without attendant losses from economies of scope and scale. This litany does not end the matter. New errors are introduced in E&H [9] and there are also other errors in the original reports [7] and [8] that are not mentioned in [8], as well as a failure to correct already existing errors. (Numbers in square brackets refer to references listed at the end of of this paper.)

For perspective, we begin by noting the serious omission of any discussion in E&H [9] on their invalid use of Shephard's lemma in [7] and [8]. To see what is involved we use C_t to represent cost in period t and p_{it}, q_{it} to represent the corresponding input prices and quantities and write

(1) $$C_t \equiv \sum_{i=1}^{m} p_{it}\, q_{it} \quad .$$

We have represented this as an identity to avoid more detailed developments and to underscore the E&H assumption that efficiency was achieved by AT&T in every year t = 1947, ..., 1977, covered by their study. Evans and Heckman then assume[1/] that they obtain access to the formulation of R. Shephard [13] and repeatedly use the following version of Shephard's lemma[2/]

(2) $$\frac{\partial C_t}{\partial p_{it}} = q_{it}$$

1/ Actually, the relations in (1) are defined in terms of frontier properties which do not hold with the statistical (central tendency) estimating relations used by Evans & Heckman in all of their papers. See [1] for further discussion and for an alternate method of estimation that can now be used.

2/ See [8] pp. 253 ff.

where the expression on the left is the partial derivative of cost with respect to the price of input i. Their use of this lemma plays a fundamental role in that it enters into the very formulations of the models used by E&H for their estimates.

It is important now to note that the observed C_t, as taken by E&H from Christensen et.al., [5] are not separately observed data but are derived instead from the p_{it}, q_{it} data via the expression (1). Evans and Heckman however moved to a 1961 base period from the 1967 base used by Christensen et.al. [5] by normalizing each input on its 1961 price but then failed to recompute the C_t values from (1) as required. See the C_t values and the p_{it} values in the 1961 row of Table 10-14 of [8] or refer to the Appendix in [2] for a detailed discussion of the way these costs were calculated from the data of Christensen et.al. See also [4].

The estimating relations derived by E&H from their translog cost function via (2) are therefore not valid. The translog terms involving p_{it} on one side of the expression are expressed relative to the 1961 prices for each input while the cost values, C_t, on the other side of these relations are expressed in 1967 prices. It follows that no reliance can be placed on any of the results reported by E&H.

In [9], p. 856, E&H state that "the discrepancy in the base for the cost [i.e., C_t] and input prices [i.e., p_{it}] ...only affects the estimated intercept of the cost function because we [i.e., E&H] adopt a logarithmic specification for the cost function." Evans and Heckman do not explicitly state how the corrections (if any) were made to obtain the results reported in [9] but this statement would indicate that they believe they could repair the situation by simply dividing

the C_t values by an index of input prices.[1/] This is not correct and if this procedure were followed it would again deny E&H access to legitimate use of the estimating relations in their models that they derived from (2).

Changing the prices by changing the base period will, in general, affect other coefficient estimates besides the intercept value. This can be shown very simply in a way that avoids detailed developments via the ordinary least squares expression[2/]

$$\beta = (X^T X)^{-1} X^T \mathcal{L} \tag{3}$$

where $(X^T X)^{-1}$ is the inverse of the product of the design matrix X and its transpose X^T. The vector $\mathcal{L}$ has as its components

$$\ln C_t = \ln \left(\sum_i p_{it} q_{it}\right) \tag{4}$$

and β is the vector of estimated coefficient values for the translog cost function utilized by E&H.

1/ See the discussion of the Table below, however where the fact that the intercept values in columns 1 and 4 are the same indicates that E&H didn't even bother to compute and apply such an index.

2/ See Chapter 2 in Sueyoshi [14].

We now complete the argument by noting that change of the C_t values to a 1961 base cannot be done by multiplying by a single factor since the factor is different, in general, for the different p_{it}. Also by (3) every component of β and not just the one corresponding to the estimated intercept will generally be affected by such changes in the $\not{b}$ vector.

We fault E&H as we did in [2] and [3] for inadequate attention to the data, including the way it was derived, and note that this fault seems to follow from a "methodological-theory bias" in which data are accorded an inferior position -- an approach which seems to have become very common in economics. This is perhaps best brought to the fore by the manner in which E&H (a) statistically test, and reject, the underlying economic theory of production they are utilizing and then (b) employ that same theory as if it were valid on the data they used to reject it. The reason E&H give for following this course is that other economists have proceeded to utilize this same body of theory without even bothering to test it. See footnote 9 on p. 620 in [7] for a list of names. This is perhaps permissible for some purposes of research of pedagogy. It is not permissible when major public policy issues are being addressed as in the U.S. Justice Department commissioned studies of E&H [8].

To provide suitable cross-checks in the future when important issues of policy are being addressed, we have not only dwelt in [2] and [3] on the errors committed by E&H but have also suggested the use of other disciplines with different methodological orientations to ensure that possible underlying biases of a methodological-theory variety are brought to the fore for explicit consideration prior to effecting policy decisions and judgments. Evans and Heckman were invited to respond to [2] so that their comments could appear in the same issue of Management Science.

Thus far, however, they have used this invitation mainly to proceed via private communications with the Editors to delay and possibly halt publication of the already accepted paper [2] in Management Science. Hence we ought to note the following, which may be considered another error of omission by E&H in [9]. Proceeding via a goal programming formulation constrained to ensure that the E&H assumptions are satisfied (and not merely assumed to be true) every one of their major results is reversed for every one of the 20 years covered in the E&H study [8] and [9].1/ Nothing is said about this by E&H in [9].

In addition to the errors of omission and commission noted above, we now turn to the Table below in which we have recorded estimates that we effected at an earlier date, which we use here for comparison with those subsequently published in [9] by E&H. The middle two columns contain estimates secured from seemingly unrelated regressions using TSP (Time Series Processor) version 3.4B (released in 1977) and version 4.0E (released in 1983) on the IBM 3081 at Ohio State University.2/ The first and last columns contain results obtained by E&H. For ease of comparison we have used the first column to list the original SUR estimates based on the uncorrected (i.e., erroneous) data as published by E&H in [8] and [7]. The last column shows the revised SUR estimates based on corrected data as published in [9] by E&H.

1/ This suggests that testing by use of alternate methodologies (to check whether results are robust to changes in methodologies) might well be added to the list of checks covered in [6].

2/ These computations and results are due to T. Sueyoshi and R. G. Anderson of Ohio State University. Two versions of TSP were used in repeated runs because different computer codes can sometimes produce different results in the SUR estimates that are obtained. See Feigenbaum, Levy and Tullock [9]. The older, 1977, version was used in the belief that this might be close to the one used by E&H and these results were checked by the later, 1983, version of TSP. Noting, with surprise, the correspondence between the published results in [9] and the estimates we previously obtained from the uncorrected data, we asked Professor Anderson to undertake yet another series of estimates and provide a report to us on his results. This report by Professor Anderson as well as the earlier one he coauthored with Professor Sueyoshi are available on request to any of the authors of the present note.

REGRESSION COEFFICIENT VALUES

FOR

BELL SYSTEM TRANSLOG COST FUNCTIONS

(Adjusted for First Order Serial Correlation)

Parameter	E - H[a]	Uncorrected Data[b]	Corrected Data	E - H[c]
Constant	9.054	9.056	9.451	9.054
Capital (Price)	.535	.527	.538	.536
Labor (Price)	.355	.361	.358	.354
Local (Output	.260	.471	.567	.206
Toll (Output)	.462	.344	.186	.504
Technology	- .193	- .203	- .011	- .201
$Capital^2$	.219	.221	.202	.223
$Labor^2$	.174	.149	.105	.174
Capital.Labor	- .180	- .176	- .144	- .183
$Toll^2$	-8.018	-8.388	-7.613	-8.969
$Local^2$	-4.241	-16.403	-8.576	-16.646
Local.Toll	11.663	11.990	8.961	12.167
$Technology^2$	- .176	- .161	.066	- .180
Capital.Toll	.337	.150	- .105	- .180
Capital.Local	- .359	.096	.162	.343
Labor.Toll	- .179	.106	.072	.161
Labor.Local	.164	- .165	- .213	- .362
Capital.Technology	.083	.015	.100	.081
Labor.Technology	- .057	- .049	- .012	- .052
Technology.Toll	-1.404	.791	2.581	1.430
Technology.Local	1.207	-1.379	-4.298	-1.553

[a]Source: Evans and Heckman [8] p. 260 and [7] p. 622.

[b]Source: Ohio State University Report by T. Sueyoshi and R. Anderson dated July-August, 1986.

[c]Source: Evans and Heckman [9] p. 857.

As can be seen, there is a close correspondence between columns 2 and 4 but not between columns 3 and 4.[1/] We are at a loss to explain how this could have occurred. Perhaps E&H believed their own erroneous statement to the effect that altering the base period used for the data calculations would not affect the resulting SUR estimates.[2/] If so, this table can be counted as another demonstration of the dangers of according primacy to theory over data. If this was not what E&H did, then the above table may be taken as a numerical illustration of what we demonstrated with (3)ff. since, as can be seen, the coefficient values vary with the data used to form the estimates.

What we call "methodological-theory bias" is exhibited in economics or at least in American economics, by the almost exclusive use of statistical regressions and index number constructions (with underlying mathematical statistical theories) like those discussed in the Table above, whenever empirical data are addressed. The goal programming/ constrained regressions as reported in [3] were, in fact, undertaken as a methodological cross-check on the results obtained in [8] and it was this alternate methodology that led to the discovery of the data deficiencies, when our linear programming codes kept reporting "no solution" (instead of the "statistical significance values" reported in the codes used by E&H).[3/] Nor does this end the matter. When these same goal programming/constrained regressions were applied to the corrected data as in [3], another result emerged: Starting in 1947, the values of the

1/ There are some exceptions to these close correspondences but the record of previous errors by E&H causes us to wonder whether it is worth according any real substance to these differences.

2/ Observe that the intercept values are the same in columns 1 and 4 which contain the E&H estimates from, respectively, the uncorrected and corrected data so that apparently E&H did not bother to check their calculations against their theorem.

3/ See footnote 1 on p. 5. See also [11].

Evans-Heckman subadditivity index obtained from these goal programming/ constrained regression estimates became increasingly negative until the mid 1960s. Thereafter the trend was reversed and the index became increasingly positive in each succeeding year.[1/] This is consistent with the findings in [12] [2/] and [15] which proceeded by a still different method, in which primacy was given to careful sifting of data accompanied by intensive examination of pertinent institutional histories and extensive consultation with industry experts. It is of interest to us that no reference is made to either of these studies in the volume edited by Evans[3/] (in which the E&H paper appeared). We also could not find any reference to these studies in the 30,000 pages and more of hearings and accompanying materials which we examined at the Telecommunications Library of George Washington University.[4/] It is for reasons like these that we have argued for more extensive use of different disciplines with different methodological orientations (or biases) when important issues of public policy are being addressed--while we still acknowledge the contribution made by E&H in publishing the data on which their results rested. See footnote 4 below.

1/ This was also accompanied by an increasing range in the low to high values in succeeding years.

2/ This study links the changes to increasing competition in the 1960s arising from situations like the Carterfone decision. See Chapter 2 in [10]. The study 13 links it to changes in technology which were blurring distinction between computers and communications, etc.

3/ Evans reported to us that he was on the staff of Charles River Associates while the study [12] was being made and the GAO study [15] which had also been in process must have been known to the Justice Department economists when they were monitoring the studies published in the Evans volume.

4/ We are grateful to the Librarian, Ms. Marry, for the help she gave us. Our efforts to secure access to Justice Department and FCC files were frustrated, however, when we were informed in an exchange of correspondence that "these materials had been returned to their authors or destroyed."

REFERENCES

[1] Charnes, A., W. W. Cooper, B. Golany, L. Seiford and J. Stutz, "Foundation of Data Envelopment Analysis for Pareto-Koopmans Efficient Empirical Production Functions," Journal of Econometrics, 1985, pp. 91-107.

[2] __________, __________, and T. Sueyoshi, "A Goal Programming/Constrained Regression Review of the Bell System Breakup," Management Science (accepted).

[3] __________, __________, and __________, " A Goal Programming/Constrained Regression Study of AT&T as a Natural Monopoly," in O. A. Davis, ed., Papers in Cost/Benefit Analysis (to appear).

[4] Christensen, L. R., "Testimony of L. R. Christensen," United States V. AT&T, C.A. No. 74-1698, Defendant's Exhibit D-T-100 (1981).

[5] __________, D. C. Christensen and P. E. Schoeck, "Total Factor Productivity in the Bell System 1947-1979, "Christensen Associates, Inc., 810 University Bay Drive, Madison, WI 53705 (1981).

[6] Dewald, W. G., J. G. Thursby and R. G. Anderson, "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project," American Economic Review 76, No. 4, Sept., 1986, pp. 587-603.

[7] Evans, D. S. and J. J. Heckman, "A Test for Subadditivity of the Cost Function with an Application to the Bell System," American Economic Review, Sept., 1984, pp. 615-623.

[8] __________ and __________, "Multiproduct Cost Function Estimates and Natural Monopoly Tests for the Bell System" in D. Evans, ed., Breaking Up Bell, Amsterdam: North Holland, 1983, pp. 253-282.

[9] __________ and __________, "A Test for Subadditivity of the Cost Function with an Application to the Bell System," in Erratum, American Economic Review, Sept., 1986, pp. 856-858.

[10] Feigenbaum, S., D. Levy and G. Tullock, "The Replicability and Robustness of Econometric Research." Center for Public Choice Working Paper, Fairfax, Va., George Mason University, 1986.

[11] LaLonde, R. J., "Evaluating the Econometric Evaluations of Training Programs with Experimental Data," American Economic Review 76, No. 4, Sept., 1986, pp. 604-620.

[12] Meyer, J. R., R. W. Wilson, M. A. Baughcum, E. Burton and L Caouette, The Economics of Competition in the Telecommunications Industry (Cambridge, Mass: Oelgeschlager, Gunn & Hain, Inc., 1980).

[13] Shephard, R. W., Theory of Cost and Production Functions, Princeton: Princeton University Press, 1970.

[14] Sueyoshi, T., "Goal Programming/Constrained Regressions and Alternative Approaches to Statistical Estimation," Ph.D. Thesis (Austin: University of Texas at Austin Graduate School of Business 1986).

[15] U.S. General Accounting Office, "Legislative and Regulatory Action Needed to Deal With a Changing Domestic Telecommunications Industry," CED 81-136, Washington, DC., 1981.

unclassified

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

REPORT DOCUMENTATION PAGE

READ INSTRUCTIONS BEFORE COMPLETING FORM

1. REPORT NUMBER
CCS 590

2. GOVT ACCESSION NO.
A193 658

3. RECIPIENT'S CATALOG NUMBER

4. TITLE (and Subtitle)
MORE ON BREAKING UP BELL

5. TYPE OF REPORT & PERIOD COVERED
Technical

6. PERFORMING ORG. REPORT NUMBER
CCS 590

7. AUTHOR(s)
A. Charnes, W.W. Cooper, T. Sueyoshi

8. CONTRACT OR GRANT NUMBER(s)
N00014-86-C-0398

9. PERFORMING ORGANIZATION NAME AND ADDRESS
Center for Cybernetic Studies, UT Austin
Austin, Texas 78712

10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS

11. CONTROLLING OFFICE NAME AND ADDRESS
Office of Naval Research (Code 434)
Washington, DC

12. REPORT DATE
March 1988

13. NUMBER OF PAGES
68

14. MONITORING AGENCY NAME & ADDRESS (if different from Controlling Office)

15. SECURITY CLASS. (of this report)
unclassified

15a. DECLASSIFICATION/DOWNGRADING SCHEDULE

16. DISTRIBUTION STATEMENT (of this Report)
This document has been approved for public release and sale; its distribution is unlimited.

17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

18. SUPPLEMENTARY NOTES

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)
Natural monopoly, production function, efficiency, translog flexible functions, econometric models, goal programming/constrained regressions

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)
See attached page.

DD FORM 1 JAN 73 1473 EDITION OF 1 NOV 65 IS OBSOLETE
S/N 0102-014-6601 |

unclassified

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

This paper consists of the following five with 1,3,4,5 by Charnes, Cooper and Sueyoshi and 2 by Evans and Heckman.

1) "Goal Programming/Constrained Regression Review of the Bell System Breakup," Management Science 34, No. 1, Jan. 1988, by A. Charnes, W.W. Cooper and T. Sueyoshi.

2) "Rejoinder. Natural Monopoly and Bell System: Response to Charnes, Cooper and Sueyoshi, Management Science 34, No. 1, Jan. 1988, by David S. Evans and James Heckman.

3) "A Preface and Post Script to Evans and Heckman Studies for the Bell System Breakup," CCS Report 588, The University of Texas at Austin, Center for Cybernetic Studies, Jan. 1988, by A. Charnes, W.W. Cooper and T. Sueyoshi.

4) "Exhibit I: A Guided Tour of A Family of Studies on the AT&T Breakup," CCS Report 580, The University of Texas at Austin, Center for Cybernetic Studies, May 1987, by A. Charnes, W.W. Cooper and T. Sueyoshi.

5) "Exhibit II: On Errors in the Evans and Heckman Bell System Breakup Studies," CCS Report 550, The University of Texas at Austin, Center for Cybernetic Studies, October 1986, by A. Charnes, W.W. Cooper and T. Sueyoshi.